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ARE EPIDEMICS CONTAGIOUS?

TO BE FOLLOWED BY

THE VOLCANIC ORIGIN OF EPIDEMICS.

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POPULAR EDITION

ARE EPIDEMICS CONTAGIOUS?

By JOHN PARKIN, M.D., F.R.C.S.

FORMERLY H.M.'S MEDICAL INSPECTOR FOR CHOLERA IN THE WEST INDIES

LONDON

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EDITORIAL NOTE ON THE AUTHOR.

DR. PARKIN died on the 18th of March, 1886, at Brighton, at the age of eighty-five, his long and strenuous life having been devoted to the study of cholera, and similar epidemics.

The present volume containing his views, with the last corrections and additions made by his hand, is published in pursuance of a direction contained in his will.

Dr. Parkin, who was a pupil of Abernethy, and was for some time under Sir Richard Dobson, M.D., Inspector of Fleets and Hospitals at Chatham, had his attention first specially directed to the subject of cholera more than fifty years ago, when visiting India and China, during a severe outbreak.

A prolonged series of observations and experiments early satisfied him that the cause of the disease was atmospheric, and that carbonic acid gas was its antidote; and when, in 1834, he had the opportunity of putting his theory to a crucial test, and on a large scale, the result was conclusive. From that time forth, any abnormal visitation of the disease, in any part of the world, was

the signal for his departure to the scene of the outbreak. In this manner, in 1834 and 1835, he visited many provinces of Spain, and especially Valencia, Barcelona, Mataró, and Cadiz. In October, 1850, cholera appeared at Kingston, Jamaica, and by December 26th he was on the spot, treating it for many months in that city, and subsequently continuing his work at Lucea, and in Green Island, Westmoreland, and Hanover. His success in this region was so notorious that when in the following year it again appeared, he was offered the post of Her Majesty's Medical Inspector in the West Indies. In 1861, he paid a second visit to India, where he remained until the disappearance of the disease.

Partly, no doubt, through indifference, owing to the comparative mildness of the cholera outbreaks in England, partly through an erroneous notion that the results of his teaching were hostile to sanitation, his theories have met with comparatively limited acceptance in England.

In Southern Europe, however, they have received abundant recognition. In the appended list will be found the titles of most of his works, and of the foreign publications in which the efficacy of his cure has been admitted. The list is, the Editor fears, very incomplete, both as regards the foregoing, and as regards Dr. Parkin's English publications, but it is given as indicating the extent of his labours, as well as the appreciation they have met with in Europe at large.

LIST OF WORKS, ESSAYS, TRANSLATIONS, &c.,

BY

DR. PARKIN.

Suggestions respecting the Cause, Nature, and Treatment of Cholera, in *The London Medical and Surgical Journal*. 1833, pp. 151-3.

Memoria sobre et Tratamiento del Cholera Epidemico. 12mo. Barcelona. 1834.

The above was translated into French by Dr. Duval, under the title: "Mémoire sur le Traitement Curatif du Choléra Epidémique," Montpellier, 1835; and into German under the title: "Abhandlung über das Heilverfahren bei der Epidemischen Cholera. Aus dem Spanischen von der Dr. Zschokke. 8vo. Aaron. 1836.

The Antidotal Treatment of the Epidemic Cholera. 8vo. London, 1836. Second Edition. 1848. Third Edition. 1866.

The above was translated into Italian under the title: "Sull' Antidoto del Cholera Epidemico. Messina. 1837.

On the Efficacy of Carbonic Acid Gas in the Diseases of Tropical Climates. 8vo. London. 1836.

This, also, was translated into Italian under the title: Sull' Efficacia del Gas Acido Carbonico delle Malattie dei Climi Tropicci. 8vo. Messina. 1841.

On Gout, its Cause, Nature, and Treatment. 8vo. London. 1841. Second Edition. 1877.

The Remote Cause of Epidemic Diseases. 8vo. London. 1841.

The Cause of Blight and Pestilence in the Vegetable Creation (*Brochure*). 1846.

The Prevention and Treatment of Disease in the Potato and other Crops. 8vo. London. 1847.

Treatment of Cholera by Carbon, and Carbonic Acid Gas. *Lancet*. 1848. Vol. ii. p. 220.

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On the Antidotal Treatment of the Epidemic Cholera. *Lancet*. 1848. Vol. ii. p. 156.

Suggestions on the Prevention of the Epidemic Cholera, with notes on the influence of locality, in the production of that disease, and on the immunity attaching to the vicinities of mineral springs, and other places, where large quantities of carbonic acid gas are evolved. 1848. *Lancet*. Vol. ii. p. 289.

Statistical Report of the Epidemic Cholera in Jamaica. 8vo. London. 1851.

Part II. of The Remote Cause of Epidemic Diseases. With Maps. London. 1853.

L'Antidote du Choléra Asiatique. 8vo. Rome. 1858.

The Causation and Prevention of Disease. 8vo. London. 1859.

The Utilization of the Sewage of Towns. 8vo. London. 1862.

The Cause, Prevention, and Treatment of the Cattle Plague (*Brochure*). London. 1875.

Epidemiology; or the Remote Cause of Epidemic Diseases, &c., &c. Part I. London. 1873. Second Edition. 1886.

Part II. of Epidemiology; or the Cause of Epidemic Diseases in the Animal and in the Vegetable Creation, with the Cause of Hurricanes. 8vo. London. 1880. Second Edition. 1886.

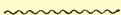
Sanitary Reform: is it a Reality, or is it not? (*Brochure*). London. 1875.

The Antidotal Treatment of Disease. 8vo. Part I. London. 1878.

Part II. of The Antidotal Treatment of Disease. Phthisis: its Cause, Nature, and Treatment. London. 1883.

Climate and Phthisis. 8vo. London. 1875. Second Edition. 1882.

ARE EPIDEMICS CONTAGIOUS?



INTRODUCTION.

DISEASES—that is to say, ordinary diseases, those not arising from individual or constitutional causes—are usually divided into two classes, epidemic and endemic. The latter comprise those maladies, that are to be met with constantly in certain countries or localities; as, for instance, ague in the alluvial districts of temperate climates, and remittent or continued fever, in those of inter-tropical regions. Epidemics, on the other hand, are those peculiar affections which, springing up suddenly in some particular spot, spread over a certain portion of the habitable globe, and then disappear altogether. After a certain interval, of longer or shorter duration, they reappear, prevail for a given period, and then subside, only to repeat the same series of phenomena again and again, sometimes for centuries. It is these peculiar maladies, commonly termed pestilences, the cause of which is now about to be considered.

Such is the object of the present work—an inquiry into the remote or *primary* cause of epidemic diseases.

There can be, to quote the language of one writer, no subject connected with the temporal interests and concerns of mankind in which, in the abstract, they are

more fearfully engaged than in the study of those diseases which occasionally ravage extensive districts, attacking with indiscriminate fury all persons susceptible of their influence, or exposed to the causes which ensure their propagation. War, however destructive in its sphere, famine, however pinching and disastrous, presses heavily but on the denizens of comparatively small surfaces of the earth. To the one, human conventions may put a period—human industry and the natural and speedy revolution of the seasons afford a certain remedy to the other. But pestilence, commencing in one quarter of the globe, soon spreads over the remainder; while human power has been hitherto unable to arrest its progress, and human skill insufficient to prevent the dire effects which have so invariably followed in its train.¹

Springing up spontaneously, it attacks, without discrimination, all persons within its reach—the rich and the poor; the strong and the weak; man in the pride of his strength, and woman in the freshness of her beauty; helpless infancy and tottering old age; the minister of religion and the votary of pleasure; the kind Samaritan, with him who passes by on the other side; and, although last, not least, the professor of that art which, if it were perfect, would prolong the life of man, not only to three score years and ten, but to ten times three score—the primitive age! Having selected its tithe of victims, it marches on to enact the same scenes, and to commit the like devastation, in other districts. When, apparently exhausted by these conflicts, it has at length slumbered for a time, and men have congratulated themselves on the

¹ History of the Epidemic Cholera.—*Lancet*, 1832.

disappearance of the destroyer, it suddenly reappears ; and, seeming to have acquired fresh strength by its apparent defeat, commences its attacks with redoubled fury. Knowing no distinction of country, and being unopposed in its progress by any barrier of art, or the natural boundaries of kingdoms, it numbers among its victims the Asiatic and the European ; the negro and the white man ; the inhabitant of the old and the settler in the new world. Not confining the sphere of its operations to the land, it traverses, with equal ease, the boundless and the trackless ocean ; visiting alike the far distant sea-girt isle, and the solitary wandering barque.

Sad and melancholy as these descriptions are, fortunate would it be, if disease and death were the only evils with which the human race had to contend at such periods. But, as if the above catalogue were not enough, man—weak, ignorant, presumptuous man—has added to the amount, by woes and sorrows of his own forging. The affrighted multitude, seeing their fellow-creatures cut off by the agency of a power equally inexplicable and extraordinary, and wishing to account for it by their own imperfect knowledge, have referred the cause to the machinations of their fellow-mortals. Hence the persecution of the Jews at one period, and the massacre of particular persons, or whole bodies of men, at another—individuals who, from accidental circumstances, were at the time the most obnoxious to popular suspicion, prejudice, and hatred. “The tendencies of the mind,” remarks Dr. Hecker, “the turn of thought, of whole ages, have frequently depended on prevailing diseases ; for nothing exercises a more potent influence over man,

either in disposing him to calmness and submission, or in kindling in him the wildest passions, than the proximity of inevitable and universal danger. Often have infatuation and fanaticism, hatred and revenge, engendered by an overwhelming fear of death, spread fire and flames throughout the world.²

Thus, in the black death of the fourteenth century, the Jews were accused by the people of poisoning the wells; and so general was this belief that, in many places, criminal proceedings were instituted against them by the local authorities, who sanctioned alike their conviction and execution. Independently of other towns—for the same tragical events were witnessed over the greater part of Germany—at Strasburg alone, 2000 Jews were burnt alive; while at Mayence, 18,000 are said to have been put to a cruel death.³ “No reasoning,” says the Rev. Dr. Ireland, speaking of the plague of Milan, in 1630, “could persuade the people that they were not suffering from the malignant agency of poisoners; and so strong was the ascendancy of this notion, that not only common friends, but members of the same family, *and even husbands and wives*, grew to be suspicious of each other.”⁴ Although, fortunately, we have not to record such acts in the nineteenth century, still, we are not entirely free from the effects of similar ignorance and prejudice. The physicians in Hungary, the agents of Government in France, and the monks in Spain, were accused of poisoning the wells, and, in many instances, murdered by the infuriated and infatuated

² Preface to “Der englische Schweiss; or, the Sweating Sickness.”

³ Hecker, on the Black Death of the Fourteenth Century.

⁴ On the Plague of Athens.

populace. Worse scenes occurred in Italy in 1867. In a letter, dated Florence, Sept. 15, the writer remarks : " Popular tumults, attended by the most atrocious acts of bloodshed, continue to take place in those parts of the southern provinces visited by cholera. The populace, in every case, appear to have been excited by their belief, that the cholera was propagated amongst them by the authorities or other persons, who either poisoned their wells, or had some means of infecting the air with the deadly epidemic. Particulars have just reached the city of a terrible scene that occurred within the past week at Ardore, a town in Calabria. Upon the first appearance of cholera at that place, the populace assembled in arms before the druggist's shop, loudly declaring their intention of burning it to the ground. There were no military at Ardore, except a few men belonging to a company stationed at Gerace. The officer in charge, Signor Garzoni, on hearing the tumult, immediately hastened to the spot, and endeavoured to dissuade the rioters from their purpose ; but the populace had lost all respect for the authorities, who besides had no means of enforcing the law. The earnest appeals of Garzoni only served, in fact, to increase the fury of the mob, who finally rushed forward, crushing the unhappy officer to death under their feet. The druggist's shop was immediately afterwards set on fire, and the whole family within ruthlessly butchered. But the thirst for blood was not yet appeased, for twenty other persons also fell victims to the ferocity of the populace. The body of the officer Garzoni," adds the narrator of the scene, " was thrown to the pigs." ⁵ The same melancholy results

⁵ Correspondent of the *Daily News*.

were observed in the new world in 1837. "In Central America," writes the editor of the *Boston Medical Journal*, "the destruction of life has been melancholy in the extreme (1837), and whole districts are represented to have been depopulated. An impression that the rivers were poisoned seems to be universally diffused, which has led to several barbarous outbreaks against the Government, hardly less fearful than the cholera itself."

Such are the consequences resulting from the ignorance and credulity of the unlearned. Painful it is to reflect, that the evils arising from, what it will be my endeavour to show are, the false views of the learned and scientific, have also been injurious and hostile to the best interests of humanity. I allude to the promulgation of the doctrine, that *contagion* is the sole and only cause of the extension, or propagation, of these maladies. From this doctrine have arisen those sanitary regulations which are productive of so much injury, and such irreparable losses, both to individuals and to communities at large. But these evils are trifling, when compared with the separation of the sick from their home and their kindred, and their isolation from all who might administer to their wants, with that sympathy which friends alone can feel or express—for the committal of their persons to hirelings, who, for the sake of gain, are induced to undertake such offices, is, to say the least, an inhuman and unwise measure. To find what the results of the prevalence of such a doctrine are, we have only to turn to the writers of the fourteenth century, one of whom, Boccacio, in detailing the horrors of the black plague of that period, states, "When the evil had become

universal, the hearts of all the inhabitants (speaking of Florence) were closed to feelings of pity and humanity. They fled from the sick, and all that belonged to them, hoping by these means to save themselves; others carried their precautions still further, and thought that the surest way to escape death was by flight. They therefore left the city, women as well as men, abandoning their dwellings and their relations, and retiring into the country; but of these also many were carried off, most of them alone, and deserted by all the world—they themselves having set the example. Thus it was that one citizen fled from another; a neighbour from his neighbours; a relation from his relations; and, in the end, so completely had terror extinguished every kindlier feeling, that the brother forsook the brother, the sister the sister, the wife her husband; and, at last, even the parent his own offspring, and abandoned them, unvisited and unsoothed, to their fate.” Once admitted as true, such opinions could not fail, as Dr. Rochoux has observed, to become the fruitful source of the most lamentable and irrational consequences. Hence, not content with establishing *cordons*, Lazarets, quarantines, purifications, &c., villages infected with the plague have been burnt, with the acclamations of the crowd, who thought to secure their own safety by these barbarous executions.⁶ Digne, in France, was only saved from this fate in consequence of the authorities ascertaining, at the last moment, that several other towns were also infected; and they naturally hesitated to apply the match to so many.⁷ But enough of such scenes! It needs not, alas!

⁶ Arrêts Notables du Parlement de Toulouse, liv. 3, tit. 7.

⁷ Noticia ecclesiæ Diniensis.

the pen of fiction, or the sober but more heartrending reality of truth, to paint the horror of such woes, in order to inflame the imagination, or to raise the sympathies of any one in the present day, when both have been so powerfully, and so recently, excited by the recurrence of catastrophes, less in degree, it is true, but similar in kind to those now detailed ; for although our own country has been mercifully spared during the visitation of the late epidemic, other countries have suffered severely, from both the ravages of the disease and the moral and social evils that this doctrine brings in its train.

This was the case in Jamaica, during the prevalence of cholera in 1850-51, as the following recital will show. Although I left England a few days only after the news arrived of the outbreak of the disease in that island, the epidemic had ceased at Kingston, Spanish Town, and that part of the island before my arrival. Finding that the disease had broken out at Lucea, on the north side of the island, I started for this place immediately. On my arrival, after a journey of three days—and that was an unusually quick one—I learnt that the disease had been prevailing between two or three weeks, although the news had only reached Kingston a few days before. Scarcely had I entered the hotel, when I was waited upon by a deputation, who designated themselves district visitors ; they had come with a request, that I would go round with them and visit some of their patients. On my asking where the resident medical men were, I was told, that one was ill in bed, that the military surgeon had shut himself up in the fort, and that the other two had retired to their country houses, and only came into the town for an hour or so in the day ! If the doctors showed this fear of the

disease, it cannot be a matter of surprise that the common people should do the same, as was actually the case. On entering the room of almost the first patient that I visited—after taking formal possession of the town, at the request of the Board of Health—I beheld, to my surprise, *a coffin* standing up in one corner. On my asking, if the patient was an undertaker or a carpenter, they replied, “Oh! dear no; but we sent for the coffin, as soon as the patient was attacked, to be ready when wanted.” “In that case,” I replied, “you do not require me; for when the undertaker is summoned, the services of the doctor are useless;” and I turned round and walked out of the house. On the friends following me, and entreating me to return, I did so, on the condition that the coffin was first removed, which was done. Fortunately, it was not required, as the man recovered. This practice, as I soon found, was universal, although, after the above scene, the people took good care not to allow me to be cognisant of the fact: the coffin was concealed, either in the house or in the adjoining premises. Direful as this practice was, there were others still worse. Directly that life was extinct, or *supposed to be*, the body was placed in the coffin, nailed down, and consigned to the dead-cart, to be carried to the common field appropriated to cholera cases. If the cart was not arrested in its course, a body might be in the grave, and covered over with earth, fifteen or twenty minutes after the last expiration. This, perhaps, would not be a matter of much importance if life were really extinct. But supposing it were not? What then? A death, the most horrible that can be imagined. That there were such, cannot be doubted: although every effort was made by me to prevent such a catastrophe.

It was stated, on more than one occasion, that noises were heard in the coffins, but the drunken drivers of the dead-cart refused to stop. No feeling of pity could be expected from men in a constant state of intoxication; for it was only under the stimulus of drink, that any one could be found to perform these duties. Never can I forget the second night passed by me in Lucea: my room, the first night, not being in the front, or facing the street. Having been engaged visiting patients from nine in the morning until midnight, I had gone to bed so tired, that it was almost too great an exertion to undress. Scarcely had I closed my eyes, when the dead-cart came rolling by, which I should not, perhaps, have heard, but for the yelling, the shouting, and the blasphemy of the attendants. No sooner had one passed than another came, so that, being unable to obtain a wink of sleep, I rose at four o'clock and commenced my rounds.

Similar scenes were enacted at Barbadoes, the first island invaded on the second outbreak of cholera, in 1854. The Commissioners of Health state, in their Report, that persons could not be found even to carry food and medicine to the sick, so great was the fear of contagion; while it was found to be almost impossible to obtain nurses, or attendants, at any cost! When the living were neglected and left to perish for the want of assistance, it is not to be supposed that the duties to the dead—sacred though they be—were better performed. In the generality of cases, the corpse would be left alone in the house, the relatives having fled, either before or immediately after the death. In other instances, as narrated by one writer—Mordichien—children would be found with the

corpse, the parents or relations having deserted both the living and the dead.

Again, the haste to bury the dead was so great, that two and three bodies would be placed in the same box, while portions of the body might be seen hanging out as the cart passed through the streets. The appearance of these vehicles, also, as I was informed by an eye-witness—for the epidemic had subsided before my arrival in this island—more nearly resembled knackers' carts than anything else. The scenes at the interments were still worse. When the cemetery became full, a piece of ground of about four acres, called Peazas, at the western extremity of Bridgetown, was purchased, and trenches dug for the reception of the dead. Into these the bodies were thrown, as if they had been dead dogs rather than human beings. This was not all. Mr. Drumm, a respectable chemist at Bridgetown, describing a visit that he paid to this spot, and the abuse, quarrelling, and blasphemy of the carriers of the dead, adds—"It was fearful and appalling, in the midst of about 160 coffins, to hear such unrestrained and unchristian language." No wonder, then, that the writer before quoted should exclaim: "Peazas! the horror of the living, the dread of the dying; it comes curdling, even now, to my memory; for I have seen the dying in greater dread of being carried there, than they appeared to be of eternity and judgment."^s Fortunate would it be if this were all; but the worst remains to be told.

It appears that the grave-diggers had 1s. 6d. for the interment of each body; and one of them, on a particular occasion, heard a noise in the coffin when about to throw

^s Mordichien.

it into the trench. "He then rested the coffin on the side of the grave, and reasoned with himself, addressing his supposed auditor within the coffin. 'If I open the coffin,' said he, 'it is night, and I cannot get any assistance for you, so that you will only live a short time, and I shall lose my shilling and a-half.' Cupidity at length prevailed, and, according to his dying confession, he buried alive the unfortunate victim of cholera and mammon."⁹ That this was not a solitary instance will be apparent by the following touching narrative, while it throws a clear light on the cause of these premature interments:—A woman at Bridgetown, as narrated by the Rev. J. Butcher, believing that her daughter was dead, sent for a coffin, and the dead-cart at the same time. The man who brought it was unable to take the body at the moment, but promised to return shortly. "During his absence, as the mother stood looking sadly on her child, she fancied she saw a movement; first in one hand then in the other, and going close to her with a light, she discerned that her eyes were open. Uttering a scream, the poor woman immediately applied stimulants, and gradually she had the happiness of seeing her daughter recover. The girl rehearses now the agony she endured when, unable either to speak or move, but perfectly conscious, she heard her mother and the undertaker speak about putting her in the coffin, and was sensible that it was laid close to her on the bed.

"If," observes the Editor of the *Lancet*, "it be a fact, as would seem to be indisputable, that during the last few weeks there have been cases,—we will not attempt to say how many or how few,—of burying alive, a

⁹ Barbadoes *Globe*, June 19th, 1854.

scandal and a horror, wholly unpardonable in the last quarter of the nineteenth century, have to be faced, and the sooner the full truth is known, and rules of safety established, the better. . . . Why not revert to the old practice, and always open a vein in the arm after death, or pass a current of electricity through before the body is finally screwed down ? ”¹ Either of these plans might be adopted in those towns in which medical men abound, but only in a certain and small number of cases ; while in country districts, and in countries where there are few medical men, if any, these precautions would be alike impracticable and impossible. The only proper and safe precaution is, to keep the body two or three days, or until partial decomposition has commenced. This can be done without the least risk, from the following among many other facts of the same kind. At Barbadoes, the majority of the cholera bodies were buried in a field at one extremity of the town ; the others in a churchyard in the town. Yet it was precisely when these cemeteries were filled with 20,000 bodies, only partially covered with earth, that the epidemic ceased. At St. Vincent, the corpses of all the cholera-patients were interred in the churchyard, situated in the middle of the town. Yet the ravages of the disease were less in this island than in any other then visited. One of the cases, previously referred to by the Editor of the *Lancet*, is the following :—

“ Dr. Canepa, a physician in Genoa, was attacked with cholera in 1844, and before remedial measures could take effect, passed rapidly into the algid stage, and to all outward seeming, ceased to live. His death was duly an-

¹ *Lancet*, August 23rd, 1884.

nounced by the practitioner in attendance. The last offices had been performed, and his family were awaiting the arrival of the undertaker in a room adjoining that in which the body lay. Suddenly the door opened, and the ghastly figure of Dr. Canepa, arrayed in burial vesture, presented itself to the thunderstruck relatives. In a feeble, scarcely audible voice, he complained of having been neglected so long (six hours in fact having elapsed since he was given up, and laid out as dead). He was immediately replaced in bed, and every restorative practised on him, till a physician could be obtained to perform Pacini's operation of hypodermoclysis; but in vain. The assistance, which might have been effective some time previously, arrived too late, and a valuable life was lost."—*Lancet*, October 11th, 1884.

The wonder is that I was not myself a victim to this inhuman practice. On the termination of the epidemic in a country district, of which I had charge in Jamaica, I was attacked with a low kind of fever, the result of fatigue and exposure to the night air, having been engaged generally from six in the morning until midnight in visiting patients over an extended area, and in a malarious district.² There were no symptoms of cholera, nothing like it, and I was then residing in the house of one of the most intelligent men on the island—a planter and an Englishman. And yet listen to what follows. Feeling one evening faint and giddy—the effect, I believe, of an overdose of quinine administered to me by a medical friend—and the attendant having left the room, I rose to open the door and obtain a little more air.³ To

² No medical man in Jamaica will venture out, in country districts, after sunset, unless in cases of great emergency, for fear of contracting fever. Nothing is so much dreaded in warm climates as the *night air*!

³ It is not generally known that large doses of quinine—medicinal

my surprise, I saw, lying on the floor of the saloon, three men whose faces were quite familiar to me—too much so; they were the carriers of the dead. Knowing their object, I said to them, You may go at once, as your services will not be required: I do not intend to die yet! They slunk away, with downcast faces, like wild beasts deprived of their prey. Had I really fainted—a probable circumstance—my body would have been wrapped in a cloth—they did not wait for coffins in the country districts—placed in the grave, and covered over in the space of five or ten minutes, long before a person could recover from an ordinary swoon. That my grave had been previously dug admits of no doubt; for this was the universal custom in Jamaica, in the country districts. Often have I been obliged, on my *first* visit to a patient, to step over the grave that had been already dug *in front* of the cottage. The custom was, as soon as the breath was out of the body—sometimes, I believe, before—to put a rope round the neck and drag the patient from his bed direct into the grave; as such, the nearer this was to the door the easier was the task.

It is not in cholera alone that premature interment occurs; the same result has been observed in other instances, and in countries in which it is the custom to bury the dead soon after death.

The *Messenger du Midi* relates the following dreadful story:—"A young married woman, residing at Salon (Bouches du Rhone), died shortly after her confinement in August last. The medical man, who was hastily sum-

doses—are sometimes fatal. This result is common enough in Italy; and it is universally ascribed to the improper administration of this drug. Such cases are popularly termed *accidentés*.

moned, when her illness assumed a dangerous form, certified her death, and recommended immediate burial, in consequence of the intense heat then prevailing, and six hours afterwards the body was interred. A few days since, the husband having resolved to re-marry, the mother of his late wife desired to have her daughter's remains removed to her native place—Marseilles. When the vault was opened a horrible sight presented itself. The corpse lay in the middle of the vault, with dishevelled hair, and the linen torn to pieces. It evidently had been gnawed, in her agony, by the unfortunate victim. The shock which the dreadful spectacle caused to the mother has been so great, that fears are entertained for her reason, if not for her life.”⁴

The late Bishop of Orleans, as he has himself related, was interred prematurely ; but being placed in the vaults of the church, a noise in the coffin attracted the attention of a passer-by, and he was rescued.

Such are the results that attend the promulgation of, and the belief in, the doctrine of contagion—results that are worse, a thousand times worse, than the evils that the practice emanating from this doctrine is intended to avert. Were I a contagionist instead of an anti-contagionist, I should blush to avow the fact, on account of the moral and the social evils that this doctrine brings in its train. “This system” (of quarantine), remarks Sir F. Maitland, “cuts up by the root all those feelings of domestic life, which are peculiarly endeared to the mind of man, in a moment of sickness and distress ; rends asunder all the usual bonds of society, and places the unfortunate patient in a situation of the most desolate isola-

⁴ The *Times*, May 6th, 1874.

tion, at the moment when the only remaining comfort of life exists in the kindness of natural friends and connections. . . . It cannot be astonishing to any man who has seen it, that even *the last dregs of hope* must expire, before any society can submit patiently to a system of discipline which can be stated, at best, as only an inferior evil to plague itself.”⁵ Whether diseases be contagious or not, it is better to stand, shoulder to shoulder as soldiers do, and meet death like men, not like cowards and poltroons. It is, besides, politic; if we forsake our fellows to-day, they will, of course, forsake us to-morrow, and we may perish by a sort of retributive justice.

These evils are not confined to individuals: communities suffer in the same way, and from the same cause. When a disease is confined to a particular city, as frequently happens, all communication with the surrounding country is, of course, interrupted. Provisions, therefore, are liable to run short, and, when this is the case, the ravages of the epidemic are invariably increased, the want of food being a most powerful predisposing cause of disease. No less powerful will be the mental depression produced with persons thus shut up in a town, and doomed, as it were, to certain death.

Referring to the influence of terror during the prevalence of the sweating sickness in Germany, Dr. Hecker remarks:—“It often happened at this time that, amidst a circle of friends, if the sweating sickness was only brought to mind by *a single word*, first one and then another was seized with a tormenting anguish; their blood curdled, and, certain of their destruction, they

⁵ Despatch addressed to Lord Bathurst, in 1819, on the plague at Malta.

quietly slunk away home, and there actually became a prey to death.”—Der englische Schweiss, p. 240.

Gafrandi, who has given a history of the plague which prevailed at Digne, in Provence, in 1629,⁶ states, that the ravages of the disease were such that, in the space of a few months, only 1500 inhabitants were left out of 10,000. He attributed this great mortality to the strict quarantine in which the inhabitants were placed, so that no one could pass the boundary line and retire into the country. Again; on the outbreak of the plague at Marseilles, in 1720, an Act of the Parliament of Aix, confirmed by the Council of State, prohibited, under pain of death, the inhabitants of Marseilles and of the suburbs from leaving the town. Hence, as we are told, a great amount of misery, and a mortality *truly frightful*.

Another evil resulting from quarantine, and the isolation of the sick, is the chance that provisions may run short, or fail altogether—a circumstance that would inevitably tend to increase the ravages of the disease. Such a result would probably have been witnessed at Lucea, Jamaica, but for an accidental circumstance. A soup kitchen had been established, to which the greater part of the population resorted; for the shops were closed, all business suspended, and nothing but biscuits and salt fish to be obtained, while even these were becoming scarce. About a week after my arrival, I was told that no meat, or live stock, could be had, as the people on the adjoining estates refused to hold any intercourse with the purveyors, or to allow them to come even on the grounds. Discussing the matter with some members of the Board of Health, one of them remarked:

⁶ Notitia eccles. Diniensis.

“It is unfortunate we cannot use *turtle*.” On my asking him what he meant, he replied: “Oh! there is plenty of turtle in the adjoining bay, but it would not, of course be right to use them, as fish has been prohibited as injurious!” “Prohibited, or not prohibited,” I said, “send for the turtle at once, convert them into soup, and hold me responsible for the result.” This was done, and the product so much relished that the people became clamorous for it afterwards. Up to that time, I had been living on salt fish and biscuits, the former half rancid, and the latter not particularly good; but turtle then became my standing dish, or rather dishes, for it was converted into a variety. During the rest of my stay in Lucea, I lived, to use a common phrase, like an alderman; so much so, that when I afterwards removed into a country district, I often looked back with longing eyes to the flesh (i.e. fish)! pots of Egypt, or, of Lucea. And what was the result of all this city feasting on this prohibited food? Not an increase, but a decrease of the epidemic, which subsided gradually from this time. The truth is, there is a great deal of nonsense written and spoken about different articles of diet, during the prevalence of cholera, so that people are actually frightened out of their wits, and know not what to eat and what to avoid. But this is a digression. It is time now to return to the subject that more immediately concerns us.

Having pointed out the evils that flow from a belief in the doctrine of contagion, let us now inquire whether any injurious results are found to arise from the opposite system—the absence of precautionary measures.

At Kingstown, St. Vincent, which I visited during the second outbreak of cholera in the West Indies, in 1854,

no precautionary measures were adopted after the disease made its appearance. Like others, they had tried to keep out the disease by isolating themselves, and refusing all communication with other islands; so much so, that I was unable to land there at that time, although sent expressly by the Governor of Barbadoes; and although, as we had heard, the epidemic was then actually prevailing at the other extremity of the island. Strange to say, it was precisely in that part of St. Vincent where there is no town, no harbour, and no direct communication with other islands, that the first cases occurred. The disease then spread from north to south, until it reached the capital, Kingstown, a few days before my arrival. By my advice, all precautionary measures were then abandoned. Not only did the sick receive every care and attention, but the body, after death, was kept a sufficient time to prevent the risk of premature interment. Instead of the dead-cart, the funeral was conducted in the ordinary way. The friends followed the body to the grave, which was in the churchyard, in *the middle of the town*, and the funeral service was regularly performed by the rector, the Rev. H. Laborde, or his curate. In one word, the inhabitants of Kingstown were treated as Christians, instead of being buried, as in Jamaica, Barbadoes, &c., like dogs or carrion crows. Did any ill results follow from this? Were these worthy professors of Christianity carried off by the plague, from the presumed exhalations given out by the numerous bodies over which they performed the last offices of humanity? Did the friends and the relatives suffer more for the kindness, the sympathy, and the respect, which they paid to the living and the dead? None of

these results followed. Not only was the mortality in Kingstown less than in any other town of its size in the West Indies, but the worthy rector and his curate both escaped even an attack.

The same beneficial results followed the abandonment of all restrictions, in some instances that have been recorded, during the prevalence of the plague in Europe. The authorities of Ferrara, in 1630, struck with the ravages of the disease in some neighbouring towns, subjected to the strictest quarantine—for the houses of individuals, in which the plague appeared, were surrounded by a *cordon*, the same as the town—caused every person, as soon as attacked, to be taken out of the house, as well as all the other inmates, and to be removed to another locality. The house was then purified and ventilated, and no one allowed to enter for a month. The good effects of this system were so apparent, that it was considered to be of public utility to record the results in the public annals of Ferrara.⁷ A similar plan was afterwards successfully adopted at Rome, in 1657, by Cardinal Gastaldi, who was charged with the sanitary superintendence of the city. It was remarked, previously to the adoption of this system, that when the disease broke out in a house, nearly all the inmates were attacked, whereas, among those who were sent away, not more than five per cent. fell sick.

If such are the reasons that can be assigned, why we should not act on the belief that diseases are propagated by contagion, what shall we say, if it can be shown that the doctrine itself is a false one? That such is the fact, it will be my object to demonstrate in the following chapter.

⁷ Muratori: Governo della peste e delle maniere de guarda sena. Modena. 1714.

ARE EPIDEMICS CONTAGIOUS?

IN the first ages of the world—periods when intercourse and communication between different and distant nations were either very limited, or else almost entirely unknown—people could have had no other theory, in order to account for the production of epidemic diseases, than that which they entertained with respect to all other, or endemic, diseases. As it would be impossible to ascribe the production of endemic diseases to contagion—inasmuch as they generally appear at particular periods of the year, and are confined to certain situations of limited extent—and, as the inhabitants of one town must have been ignorant, when an epidemic sprung up, that the same disease had previously visited other localities, or that it had subsequently spread to distant regions, contagion, or infection, could not then, we may presume, have entered into their minds. Hence the aphorism of Hippocrates—*aer est omnium rex morborumque causa*, applied equally, at that period, to epidemic and endemic diseases. When, however, commercial traffic and social intercourse became more extensive and frequent, and the occurrences which took place in one country or town were known to the inhabitants of other regions, a different opinion began to be entertained. Observing that epidemic diseases are

characterized by their universality and extent, and by bringing under their influence, not a few individuals in some single locality, but a large portion of the human race, certain theorists attempted to show, that these diseases were produced by human, not by natural, agencies. This was more particularly the case during the prevalence of the Black Death—a disease that sprung up in the East, and then spread over the whole of Asia and Europe during the fourteenth century, and when, as it would seem, the doctrine of contagion was first generally promulgated.

That certain local diseases were considered to be contagious ages before this, we have proof from Holy Writ. Lepers, for instance, were enjoined, by the law of Moses, to separate themselves from their companions; and the rules for their treatment are alike precise, complete, and severe. But there are no instances on record of the application of the doctrine to epidemic diseases, or of the isolation of the sick during their prevalence. And yet, the history of the several plagues that occurred, during the sojourn of the Israelites in Egypt, has been given. The Arabian physicians, also, who had such frequent opportunities of observing and investigating the plague, regarded it simply as an epidemic, due to the same causes as those which gave origin to other, or endemic, diseases. It was not until the middle of the sixteenth century, that the doctrine of contagion or infection was established, having been promulgated and formulated by the celebrated Verona doctor, Fracastor. This doctrine, unlike the majority of other doctrines and theories, was not only eagerly adopted at the time, but it has continued, with slight modifications, to the present day. The reason is

clear. It offered a very easy solution of a most difficult problem, for as there is said to be a cat in every house, to whom all untoward accidents are usually ascribed, so, also, there must be one or more persons in every situation, on whom the sin of the propagation of epidemic diseases can be readily laid. It is, in fact, the old story of the witches, to whom everything was ascribed that could not be otherwise accounted for. But, as it would be wrong to allow either the cat or human beings to be unjustly accused, it will be desirable to ascertain on what foundation the doctrine of contagion is based.

According to the Fracastorians, the cause of all pestilential diseases is a specific virus, which is given out by a sort of exhalation from the bodies of the sick, infecting, to a certain but limited extent, the surrounding air. They also conclude, that the direct contact of persons and things with a diseased body is sufficient to propagate the disease. Hence, there are two methods, according to these theorists, of spreading infectious diseases, by mediate and immediate contact. As to the nature or quality of the virus, no explanation was offered, and yet this is an important consideration. If diseases can be propagated by the inspiration of an infected atmosphere surrounding the bodies of the sick, the poison must be a gaseous or volatile substance, or, at all events, a substance of a specific gravity less than that of atmospheric air. If, however, these diseases are produced by direct contact with a diseased body, the poison ought to be in a fluid or solid state; otherwise it would not, we may presume, be retained either by animate or inanimate substances; more especially if the statements made, on this part of the subject, bear the least semblance of truth.

Thus we are told, that the virus of contagion attaches itself to certain bodies, called *contumacious*, which not only allow of its transport to any distance, but which are susceptible of preserving it intact for thirty or more years. If such be the case, then the *virus* must be a solid substance. Are we then to assume, that the virus is given out from the bodies of the sick under two different forms, that of a gaseous or volatile, and that of a fluid or solid substance? This is possible, but it is not probable.

If unable to determine the exact nature or quality of the agent productive of epidemic and pestilential diseases, we shall be still more puzzled to account for the manner in which it produces its effects. If these diseases be propagated from individual to individual, and by mediate contact, there must be numerous instances in which only a small portion of poison can be imbibed—the contact of the healthy with the sick being sometimes only slight, temporary, and accidental. So, also, if a piece of rope, a morsel of straw, or a flower, sent by a lover to his mistress in a letter, be sufficient to produce, and have in fact produced, as we are gravely informed, an attack of plague; an homœopathic dose only of the poison could have been inhaled or absorbed by the individual who handled these articles.

On the other hand, if a minimum quantity of the poison can produce an attack of plague or other disease, those who are in constant attendance on the sick, and who, consequently, must have imbibed a larger quantity of the deleterious matter, ought not only to be attacked in larger proportion than other classes, but also with greater intensity. This, however, is not the case; the contrary

is, rather, the fact. In the next place, those who have been accidentally and temporarily exposed to some supposed source of infection, and who could only have received a minimum dose of poison, have had the disease in as severe a form as others. Hence it has been inferred, that the virus of contagion, when introduced into the blood, reproduces itself, more especially as some persons, exposed to a presumed source of infection, have not been attacked until many days—six, seven, eight or more days—after. The process itself was compared to that of fermentation—the blood being the *must*, and the virus the *ferment*. In this way, a small quantity of leaven, or poison, would be sufficient to leaven or render poisonous the whole mass—a doctrine that received a considerable impulse after the discovery by Schwann, in 1836, of the yeast plant. If the ordinary process of fermentation, it has been argued, be the result of the presence of a living organism, an explanation is at once afforded of the multiplication of the virus within the system—the great characteristic of these vegetable organisms being their rapid development and propagation in an appropriate pabulum.

This theory, so simple and so fascinating, has found numerous advocates, not only formerly but in the present day—a circumstance not to be wondered at when we remember, that it is the oldest of all theories—as old as the time of Hippocrates. The latter, however, although concluding that diseases were produced by a sort of fermentation, thought that the poison was derived from the air, not from the bodies of the sick. Even chemists have entered into the arena, in order to do battle, on this vexed question, with those to whom the subject more

particularly belongs. Thus Baron Liebig, who, since the death of Davy and Faraday, may be regarded as the modern apostle of chemistry, thus expresses himself:—"A substance, in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid." And Sir Thomas Watson, alluding to the opinion of Liebig, remarks:—"In order, therefore, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system, which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's wort) should be present in the blood, and this ingredient must have a definite relation to the given poison." And Sir Thomas adds, in another place:—"Thus the virus of smallpox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid; and whilst the process is going on, the natural working of the animal economy is disturbed: the person is ill. The transformation is not arrested until the whole of that ingredient in the blood, which is susceptible of the decomposition, has undergone the metamorphosis."¹ Mr. Simon, who draws the same conclusion, remarks:—"By vaccination, therefore, all the substance, susceptible of change by the virus, becomes so altered, that an additional quantity can produce no further change, so the person becomes insensible to its influence."

Several objections can be raised to these conclusions. If all the ingredients in the blood, susceptible of change, undergo decomposition, on the introduction of a certain

¹ Principles and Practice of Physic, vol. ii., p. 790.

portion of the specific poison into this fluid, how does it happen, we may ask, that the effects resulting from vaccination and inoculation are so slight, when compared with the natural disease? In the instances referred to, we have ocular demonstration that a poisonous, or morbid, matter is introduced into the blood; and yet only a single vesicle or a few pustules will be produced, while, in the natural disease, the body will be covered from head to foot with a pustular eruption! How, then, is this difference to be explained? It cannot be explained by a reference to this fermentation theory. Were this doctrine true, all contagious diseases ought to be invariably fatal: the fermentative process, when once set up in the system, would go on until all the *must* or ingredient in the blood, susceptible of change, had been converted into *ferment*. The fluid in the blood vessels would then be no longer blood, but a new compound; and, as no other compound can perform the functions of the vital fluid, death would be the result. We have proof, in fact, independently of induction, that this fermentative theory is a false one; blood has been drawn in all diseases and in all stages of every disease, but no one ever saw that fluid in a state approaching even to that of fermentation. On the other hand, no alteration has been found in the constituent elements of the blood: a change of colour, owing to the greater or less degree of oxygenation, being alone observed. We may therefore conclude that this fermentation theory is an erroneous one.

There are, however, two species, or groups, of fungi, the yeast, or ferment plants, and the putrefactive fungi, which are developed during the process of putrefaction. It is to the latter species that certain writers have re-

ferred the production of cholera and other diseases. That fungi have been found in the stools of cholera patients is undoubted; a result that might have been inferred *à priori*, these plants being rapidly produced in substances that have undergone only a partial decomposition. The mucus and other organic substances present in the evacuations will necessarily, when exposed to the air, undergo decomposition, and the putrefactive fungi be immediately developed—an effect that appears to take place in an incredibly short space of time. The spores of fungi are so light that they float in the air; and as they appear to be universally diffused in this fluid, they will be deposited on everything, ready to be developed in their appropriate medium. Dr. Beale remarks:—"That fungi are not essential, and that they result from the development of germs derived from without, coming in contact with the discharges already in a state of incipient decomposition, seems proved by the fact, that specimens of the secretions in which they usually abound, and in which all the essential morbid characters are remarkably distinct, may be obtained, which are altogether free from bacteria and fungi; while, if these very specimens be kept for some hours, at the temperature of the body, bacteria make their appearance."² As, also, some of the matter thrown off from the intestinal mucous surface must be in a state of partial decay, even before it passes out of the body, there is no reason why fungi should not be developed within, as well as without the body, under such circumstances, if the spores be present. That such is the case has been

² Microscopical Researches on the Cattle Plague. By Lionel S. Beale, M.B., F.R.S. Appendix to Third Report of the Cattle Plague Commissioners.

generally inferred. It can hardly be otherwise, if the spores exist constantly in the atmosphere, as we are certain is the case; for the production of these plants, at all times and under all circumstances, cannot be explained in any other way. If, therefore, spores be constantly present in the atmosphere, the probability is, that they would enter the system with the air inspired. According to the preceding author, "these vegetable germs (bacteria) have been found in the alimentary canal, and in the interior of the epithelial cells of the mucous membrane of the intestines, in cholera; as, also, in other and slighter affections." They have also been found in the blood vessels, both in this disease and in the cattle plague. Hence Dr. Beale infers, that the spores exist in the blood and tissues at all times. But their presence there will be perfectly innocuous, unless they meet with an appropriate pabulum—decomposing matter. As this can only occur under particular circumstances, in states of disease, not of health, the fungi must be regarded, not as causes, but as effects of some other and antecedent cause.

The same arguments will apply to other diseases, which have been referred to the same cause, and with apparently more reason, inasmuch as fungi exist in these to a much greater extent than in the epidemic cholera. This is the case with the disease among cattle termed "the blood," and also with the "malignant pustule" in man and animals, which have been referred by M. Davaine to the presence of vegetable organisms—bacteria or *vibriones*.³ As, however, the spores of these fungi exist in the air at all times and under all circumstances, and as these dis-

³ Comptes Rendus, August, 1864.

eases are only developed at particular times, and with particular individuals, it shows that the presence of these vibriones in the system is an effect, not a cause, of the morbid state of the blood that then exists. These conclusions have been confirmed by some experiments of MM. Leplat and Jaillard. They found that rabbits, inoculated with the blood of a cow that had died of anthrax, or carbuncle, were attacked by the same symptoms, and died. Nevertheless, it was impossible to discover, after death, the slightest trace of bacteria, or fungi, in the blood, or in the secretions of these animals. The converse of this experiment was then made. A clot of blood was washed and separated from the supernatant liquid, and the rabbits were inoculated with some of the latter fluid, swarming with bacteria. Not the least symptom of disease was produced. They therefore concluded—1st. That the presence of bacteria in the blood or in the secretions is merely an accidental circumstance (*épiphénomène*), and cannot be considered as a cause. 2ndly. That the blood of the spleen (in the disease called “the blood,” *sang de rate*) is the more infectious, when it contains less bacteria.”⁴

That singular and destructive disease, which has prevailed of late years among the silkworms in France, and termed *Pébrine*, has also been referred by Pasteur to the presence of those organisms termed *Psorospermia*.⁵ Notwithstanding the labours and the researches of this distinguished chemist, who spent some years in investigating the cause of this alarming disease, which threatened

⁴ Comptes Rendus, 1865, p. 298.

⁵ Sur les Maladies des vers à soie.—Comptes Rendus, 1865, p. 506.

the total destruction of the silkworm in France ; and notwithstanding that the conclusion of Pasteur has been accepted by the majority of scientific men, we may yet be allowed to question its validity. If this disease be due to the presence and the multiplication of these parasites in the bodies of the silkworms, how is it that *pébrine* was never observed before 1840 ? This disease was then as new among this species of the animal creation as the epidemic cholera was with man, in 1832. As these organisms must have been co-eval with the silkworm, we have a right to conclude, that the same result would have occurred long before, if it were due to the operation of such a cause. That these parasites existed in large numbers has been clearly shown by the microscopical researches of Pasteur ; but, then, we must regard them simply as effects, having been developed by a morbid change or decomposition of the tissues, produced by another and antecedent cause.⁶ It has been shown, in fact, that the healthy silkworm is proof against the attacks of *oïdium albicans*. But when from any circumstance the vitality of the insect becomes lowered, fungi appear ; and that peculiar affection termed “ muscadine ” is produced. That a change in the properties of the blood is the primary and necessary initiator, in these cases, is admitted by one of the best authorities on the subject. M. Robin remarks :— “ The circumstances, that appear to be favourable to the development of muscadine, are those which produce, in the first instance, an alteration of the humours or of the organs of the animal, while alive ; and it is in consequence

⁶ The bodies of the silkworms were covered with black or gangrenous spots, and hence the term *pébrine*.

of this alteration that the parasite appears." And the writer then adds: "The results of the artificial introduction of the spores (into the blood) are not in opposition to the preceding conclusions; for the puncture made to introduce the *mycelium* and the spores (into the blood) is sufficient to produce those changes in the humours, and then, by degrees, in the whole economy."⁷ These conclusions are confirmed by a fact, stated by M. Andouin, viz. that muscadine can be generated at the will of the operator, by merely placing the animals in an unhealthy condition,—by shutting them up and feeding them in close bottles and boxes.

That fungi do not and cannot produce disease in the animal creation, we may be assured from the fact, that many species form articles of diet with man; while, as Dr. Beale has remarked, "some animals devour fungi in every mouthful of food they take, yet these animals live and thrive on these terrible agents."

Instead of vegetable organisms, epidemic diseases have been referred, since the time of Linnæus, to animal organisms. Remarking that the blight in vegetables was always accompanied by swarms of insects, and regarding the latter as the cause of the former, this celebrated botanist inferred, that epidemic diseases in man were also the effect of the same insectile agency. This opinion has been advocated by other writers, and particularly by Sir Henry Holland, in an Essay, published in 1839.⁸ He remarks:—"The question is, what weight we may attach to the opinion that certain diseases, and especially some of an epidemic and contagious kind, are derived

⁷ *Vegetaux Parasites*, p. 585.

⁸ On the Hypothesis of Insect-Life as a cause of Disease.

from minute forms of animal life existing in the atmosphere under particular circumstances, and capable, by application to the lining membranes, or other parts, of acting as a virus on the human frame." This theory, like the vegetable one, was advocated for the same reason as the latter, viz. that it offered an explanation of the problem, the presumed multiplication of the virus of contagion within the body. That might be, but, then, this is merely the enunciation of one hypothesis in order to prove a previous one, for this multiplication of the virus within the body is an entirely gratuitous assumption.⁹ Granting, however, that it be the fact, we should then have to prove the presence of these insectile agents in the system. But this proof has never been afforded in any epidemic or contagious disease. On the contrary, Sir Henry Holland states, that these destroyers of the human race are "minute, beyond the powers of all sense."¹ Having thus an invisible enemy to cope with, it will be better to leave him until he manifests himself to us poor mortals in some tangible shape. We have enough to do in combating those that are visible!

Pacini and Beale referred the production and propagation of epidemic diseases to the very lowest organisms, or, rather, to organic particles. They exist, we are told, in the blood, but can only be discovered by the aid of a high magnifying power. This new doctrine has received the name of the germ-theory of disease. "A disease germ," according to Dr. Beale, "consists of soft matter in a living state, and behaves in a manner peculiar to matter which is alive. It is less than the

⁹ As Aristotle remarks : *Posito uno absurdo multa sequuntur.*

¹ Medical Notes and Reflections, 2nd Edition, p. 583.

$\frac{1}{100,000}$ th of an inch in diameter, and is often so very clear and structureless as to be scarcely distinguishable from the fluid in which it is suspended. It is further inferred that these germs have been detached from already existing living matter; that is to say, from the germinal or living matter of the blood. If so, there must have been some antecedent cause in operation for the production of this effect, it being a morbid one. But no reference has been made to this part of the subject. And yet, this is the most important point after all, for as there is no effect without a cause, the discovery of this would have enabled us to ascertain whether the microscopical effects observed in the blood were due to the cause assigned or to some other process. The probability is, that these particles, if morbid ones, are not the cause of the morbid action, but simply effects of the same cause as that which produces the disease.

As regards the mode of propagation of diseases by these particles we have more ample information, or, rather, more hypothetical conclusions. "Disease germs," continues the above author, "are liable to be suspended in the air we breathe, or they may be disseminated through the water we drink, or hidden in the food we eat." It is principally through their suspension in the atmosphere, that these germs are supposed to find their way into the system, either by means of the inspired air or through the skin, the mucous surface of the conjunctiva. But, in all cases in which disease germs produce their characteristic effects, they reach the blood. Until they have entered this fluid there is no possibility of their exerting any deleterious effects upon the system.

Dr. Beale remarks: "These poisons (the disease-germs)

not only seriously derange the healthy functions, but, having entered the body, they multiply many millionfold. They are living, and increase as living particles alone increase; they grow, they feed upon the nutrient juices of the organism, and upon the tissues, and, in some cases, flourish at their expense and destroy them. The poison, when it enters, may be so infinitesimal in quantity, that it can neither be measured, weighed, or under ordinary circumstances, seen, but having gained access to the blood and tissues, it increases to such an extent, that in many cases, sufficient is produced in one subject to infect hundreds of persons—the population of a town, or even a whole country.”² Not only is all this pure hypothesis, but the conclusions are the most gratuitous, the most unwarranted that were ever made by a scientific observer. That particles or corpuscles exist in the blood, is undoubted, but not the slightest proof has been afforded that these particles are morbid products or the germs of disease. There is nothing, in fact, to distinguish them from the other particles in the blood, either by their colour, their form, or other peculiarity. This has been commented on by another writer, who observes: “Dr. Beale has examined portions of infected blood, textures, and mucous discharges, with the highest magnifying powers that exist, viz. one-fifth of an inch focal adjustment. This magnifies 2800 diameters, or, to express the magnifying power by some examples, an inch would appear to extend over 111 yards, and a child, three feet tall, would look as high as Mont Blanc. With such power, particles of even $\frac{1}{100,000}$ th of an inch in diameter, having any distinct character, would not be passed over. But he has

² *Monthly Microscopical Journal*, October, 1870, p. 205.

found no definitely formed substance that can certainly be said to be the cause of the cattle plague. He finds a great increase of granular matter, but no new appearance decidedly characteristic of the disease.”³ These conclusions have been confirmed by Dr. Beale himself, who states, “That a minute particle of the bioplasm of diseased tissues resembles in every particular, that we can ascertain by observation or experiment, a minute particle of healthy living bioplasm.” And he then adds: “I think we shall find ourselves compelled, by the necessities of the case, to refer the properties of these different substances, to what must be termed a difference in vital power.” This, to say the least, is a very extraordinary confession. We are first told that these particles are disease germs, and then, when we expect to have them described, and the differences between them and healthy bioplasm pointed out, it is stated that they owe their property to a difference of vital power! If this be the conclusion to which Dr. Beale has been driven, by the necessities of the case, he must have been sadly in want of an argument to substantiate his theory.

Although it would hardly seem necessary to discuss the validity of this theory any farther, it may be as well to add, that its advocates dwell much on the circumstance, that it offers a ready explanation of the presumed increase of the virus within the body. But the explanation thus offered in support of the doctrine, the same as that advanced for the fermentation theory, is fatal to it. If these organisms can and do reproduce themselves, when present in the blood, in the tissues, and in the secretions, there can be

³ Third Report of the Cattle Plague Commissioners, p. 6.

nothing to prevent their self-multiplication *ad infinitum*, short of their destruction or that of the patient. As it would be folly to suppose that these germs could be destroyed by any means with which we are at present acquainted, if it be true that a cubical inch of water, and consequently of blood, contains millions of particles, they would go on increasing as long as any papulum, or nutrient portion of the blood, remained. Before this was effected, the patient would die and these diseases be invariably fatal. But this is not the ordinary result. Fatal as pestilential diseases sometimes are, the half or even two-thirds will frequently recover. No such result as this would or could be witnessed, if these Liliputian cannibals were present in the body.

That the theory is an erroneous one may be shown in another way. If a patient, who has been attacked with plague or cholera in one locality, removes immediately to another, in which the causes productive of the disease are not in operation, he will immediately get rid of the disease, even without the aid of medicine. M. Penay, surgeon of the 3rd cavalry regiment, stationed at Neguillè, lower Egypt, during the epidemic of 1841, states: "That they had twelve plague patients dangerously ill, when they received orders to leave for Zagazig. They placed on board boats the healthy and the sick together, and as soon as the patients began to leave the epidemic focus, they experienced an immediate amelioration of their symptoms, and when they arrived at Zagazig, after a voyage of ten days, nearly all were convalescent, one only having died." Again, the army under the command of the Marquis of Hastings was attacked with cholera, in 1819, while encamped on the banks of the Scinde. "Unsubjected to the laws of contact

and proximity of situation," to quote the writer of the Bengal Report, "which have been observed to mark and retard the course of other pestilences, it surpassed the plague in the width of its range, and outstripped the most fatal disease hitherto seen, in the destructive rapidity of its progress. In the course of a week it had overspread every part of the camp, during which brief period, of 7000 fighting men 761 fell victims to the disease, while it was conjectured that 8000 of the camp followers, or one-tenth of the whole, were cut off. It was then wisely resolved to change the encampment, and although the line of march was covered with the dead and the dying—men dropping from their horses or falling while marching in the ranks, as if struck by a cannon-ball—they succeeded, after a few intermediate halts, in reaching the high and dry banks of the Betwah, at Erich. Here they almost immediately got rid of the disease, not a severe case having occurred after the third day."

Notwithstanding these facts the outbreak and spread of cholera in London, in 1866, was ascribed solely to the action of these particles or germs. As may be remembered, the epidemic expended its fury, that year, principally on the East End of London, in the area supplied by the East London Water Company. This circumstance was not likely to be passed over by those who adopt the water theory, and by those illogical theorists who conclude, without thought or examination, that coincidence is cause. Attention was first directed to the subject by Dr. Farr, in the weekly returns of the Registrar-General; and he and other writers, having inferred that the water had become contaminated, four different commissions were appointed to inquire into the validity of

the charge. These were, the River Commission and those of the Privy Council, of the Board of Trade and of the *Lancet*. It is unnecessary, on the present occasion, to produce any of the arguments or conclusions of these investigators : suffice it to remark, that the result of the trial was a verdict of guilty pronounced against the River Lea, from which the East London Water Company drew its supply. This is not all. The extension of the disease from the east to the west of London was also referred to the same source by the contamination of the water in the Thames.⁴ "It may at first sight appear impossible," remarked Dr. Farr, "that the cholera flux of one or more patients should produce any effects in the waters of a river like the Thames. But living molecules, endowed with the power of endless multiplication, are inconceivably minute and may be counted by millions in a drop of water." But this endless multiplication is, as previously remarked, fatal to the theory, even supposing that these molecules be actually present in the water, which however, has not yet been demonstrated.

The subsequent history of the epidemic also proves, that this water theory is entirely fallacious. Had the contamination of the water, in the River Lea, been the sole cause of the propagation of the cholera, something like uniformity ought to have been observed, in the different districts to which this water is distributed. But nothing could be greater than the variation in the rate of mortality, in the different districts supplied by the East

⁴ This visitation was honoured by two reports, or histories—one by the Registrar-General, drawn up by Dr. Farr, and another by the then Medical Officer of the Privy Council—although there was nothing peculiar in it, and although the facts themselves lie in a nut-shell. The rest is all hypotheses, and —.

London Water Company, in 1866. The deaths varied from 171 to 10,000 living in the district of St. John, George-in-the-East, to seven in East Haggerston, and to only three at Stamford Hill. In some of the sub-districts supplied by this company, as Lower Forest, Wanstead, Walthamstow, and Buckhurst Hill there were neither deaths nor attacks. Finally, in North Woolwich, which obtained its water solely from the same source, not a death from cholera and not a case of the disease occurred. Dr. Letheby, the then city medical officer of health, and who, previously, was a believer in the production of cholera and its propagation by means of impure water, remarked in his Report to the Commissioners of Sewers : " While there is ample proof of the propagation of choleraic disease by certain well waters of London, there is at present no evidence, that the public water supply has had anything whatever to do with the localization or spread of the recent epidemic. Already, in the eastern parts of London, where the disease was most fatal, the medical officers of health have reported to this effect and have shown, that the largest mortality from cholera was of persons who were not water-drinkers, and that teetotalers and others, who drank largely of the East London water, in its unboiled state, have been singularly exempt from the disease. At the City of London workhouse, at Bromley, where the inmates partook of well water alone, there were twenty deaths from the epidemic in one week ; whereas, at the East London workhouse, at Hackney, which is supplied with the East London Company's water, there has not been a single death from cholera." With these facts before us we are bound to conclude, that in the outbreak of cholera in

the East End of London, in 1866, propagation was not caused by the contamination of the water in the River Lea.

Nor could the spread of the disease, in the other districts of London, be referred to the presence of germs or other infectious matter in the water of the Thames. Had such been the case, we should have found that, next to the water of the Lea, the ravages of the disease were the greatest, if not the only ones affected, in the districts supplied with this water. But the reverse is the fact. This will be rendered evident by the following table compiled from the Registrar-General's report:—

Name.	Source.	Per 1000 Living.
East London	River Lea	70·50
New River	Springs ⁵ .	8·72
Kent Company	"	15·30
Southwark and Lambeth	Thames .	5·89
Other Companies . . .	"	3·60

These statistical facts are sufficient to prove, irrespective of all previous arguments and conclusions, that this infective water theory is alike unsound, illogical, and false.

Nor could the epidemic have been propagated by means of these presumed living particles, in any other way, as by personal contact or through diffusion in the atmosphere. Excluding the East End, the number of victims from cholera and diarrhœa in London, in 1866, was 4410. Allowing three cases to each death, there would thus have been 13,000 infected persons scattered over the various districts, irrespective of the districts supplied by the water of the River Lea. Including the latter, there must have been nearly 20,000 patients

⁵ Draws part of its supply from the River Lea.

affected either with cholera or diarrhœa that year, and of the disease, we are told, propagated as readily by the latter affection as by the former. As such, if two patients, the number to whom the diffusion of the cholera was ascribed that year, could infect 20,000, how many we may inquire would 20,000 infect? A school-boy can answer the question, while other persons will know, without making the calculation, that London, if this doctrine were true, would have been half depopulated in 1866, by these anti-malthusian entities. Yet such is the doctrine that was propounded and adopted by those who were considered to be scientific investigators.

It might have been supposed, after the preceding facts, that there would have been an end of the germ theory of disease, but another has been lately proposed. Dr. Koch, a few years since, inferred, that a *bacillus*, found in the lungs in cases of phthisis, is the cause of this disease; and he has since concluded, that the same organism is the sole cause of the epidemic cholera. It having been my object to show, on a previous occasion,⁶ that the former hypothesis is alike illogical and unsound, it only remains to consider on what foundation the latter conclusion rests.

Dr. Koch infers, that the bacillus, productive of cholera is a specific one, and acts upon the digestive organs; that its germ is developed by moisture, multiplies in water, with almost infinite rapidity, and is then distributed by the medium of some liquid or by drinking water. As regards the specific character of this micro-organism, on which Dr. Koch lays so much stress, he stated, in his first report, dated Alexandria, September 17th, 1883: "These bacteria are rod-shaped, and belong

⁶ See Part ii. of the Antidotal Treatment of Disease.

accordingly to the genus bacillus. They resemble most nearly, in size and form, the bacilli found in glanders." In a second report, dated Calcutta, January 9th, 1884, the writer remarks: "The microscopic examination demonstrated the presence of the same bacilli in the cholera intestines as had been found in Egypt." If so, there could be nothing specific about this organism, it being also found in cases of glanders. In a subsequent report however, only a month after, and dated February 2nd, 1884, we are told, "That the bacilli are not quite rectilinear, like other bacilli, but slightly curved, like a comma. The curvature is sometimes sufficient to give the bacillus a semi-circular form." It is to the latter form of bacillus, that Dr. Koch now attributes the disease; why, it would be somewhat difficult to say as there is nothing specific about this bacillus, more than with the rod-shaped one, as it has been found in the secretions of the mouth in healthy persons. This was first pointed out by Dr. Lewis, in one of the army English medical reports, and has since been acknowledged by Dr. Koch himself. There can be no reason, therefore, for ascribing the cholera to the action of the comma-bacillus more than to the rod-shaped one or any other form of micro-organism, and there are many and of different kinds found in the intestines of cholera patients after death.

Had it been otherwise, or had these comma-bacilli been the only ones found in the bodies of cholera patients, and had we proof, which, at present, is entirely wanting, that these organisms are constantly present in the intestines during life, as well as after death, no inference can be drawn from these facts. It is a law, characteristic of all parasites—animal as well as vegetable—to

attach themselves to particular substances only. Thus, wheat has four varieties of fungi, which attack different parts of the plant—the leaves, the flower, the grain, and the stalk. If, therefore, bacilli be constantly present in the intestines, or the stools of cholera patients, it is simply because they there find their appropriate food. As, however, these organisms are not found in the intestines during health, but only during certain morbid conditions, they must be regarded, not as the cause, but as the effect of the disease. This inference is confirmed by other facts.

That the presence of these or other microbes in the intestines are not, and cannot be the cause of cholera can be shown in a variety of ways. In the first place, it may be remarked, this peculiar affection is a disease *not* of the intestines but of the blood. The pathological or, rather, physiological, characteristic of the epidemic cholera is the stagnation of the circulation, in the first stages, and its total suspension in the last stage of the disease; the separation of the serous part of the blood, and its escape into the intestinal tube by the abdominal veins. The intestines are entirely passive in this peculiar affection, and merely serve as a conduit for the escape of the serum, after its separation from the solid constituents of the blood. Had chemical analysis not confirmed the conclusion, common sense would have told us, that the characteristic discharges in the epidemic cholera were composed of the serum of the blood, from the simple fact, that all the secretions are suspended during the collapsed stage. As this separation of the serum is a morbid effect, and is not observed in ordinary cases of congestion of the venous system, it could only be produced by the intro-

duction of a morbid agent into the blood. This conclusion is confirmed by the fact, that the only other disease in which this peculiar effect is observed is in cases of malignant ague, an affection caused by the introduction into the blood of a well-known poison malaria. If, therefore, bacilli be the cause of the epidemic cholera, they must be introduced into the blood, otherwise no effect could be produced. But we have been told by Dr. Koch, that the blood is free from micro-organisms; the ejecta also contained very few, but the excreta contained a significant amount. These conclusions granted, Dr. Koch's theory falls at once to the ground. No one, in fact, acquainted with the pathology or, rather the physiology, of this *nova pestis*, could have thought, for a single moment, of broaching so absurd and illogical a theory.

Nor would any one, acquainted with the past history of the epidemic cholera, have taken a trip to Egypt and a voyage to India, in search of a specific bacillus; or, when found, have thought of ascribing the propagation of this disease, from individual to individual and from country to country, to the multiplication of these micro-organisms and their diffusion in drinking water. As regards the former hypothesis—the continued and increasing multiplication of such entities *ad infinitum*—this very circumstance is, as previously shown, fatal to the germ theory of disease. Independently of the arguments then advanced, it may be remarked, that, if this hypothesis were true, the cases ought to present a mild form at the commencement of the outbreak, and a more malignant form at its termination. But the reverse is generally the fact. For example, of ninety-eight patients admitted into the *Hôtel-Dieu*, Paris, during the first

three days of the visitation, in 1832, no less than ninety-three died—ninety-five per cent. So, also, of 509 cases of cholera, admitted into the London Hospital, in 1866, the mortality was at the rate of eighty-five per cent. in the first week, and only thirty-five per cent. during the last week. The attack, also, in some instances is so sudden, and subsides so rapidly, that there is no time for the multiplication and spread of living entities. Thus, at Punderpoor, the invasion was so sudden, that people were seen tumbling over each other in the public streets as if struck by lightning, while the epidemic only lasted three days. In the upper provinces of India, the disease seldom lasted more than three days, during the first years of the prevalence of this epidemic in India. To talk, therefore, of the propagation of cholera under such circumstances and by such means is not only an absurdity but an insult to science, if not to common sense.

No less fatal to the theory of Dr. Koch is the conclusion, that the spread of cholera is due to the introduction of these bacilli into the drinking-water. Port of Spain, Trinidad, derives its supply of water from the surrounding mountains, each house being connected directly with the reservoir placed 400 or 500 feet above the level of the sea. The contamination of the water, from human sources, is impossible; yet this town suffered as severely from the ravages of the epidemic cholera as those towns that derived their water from wells. Take another fact. While proceeding up the China Sea, in one of the late East India Company's ships, we were suddenly attacked by cholera, men falling on deck as if struck by lightning. This continued for three days, when the visitation as suddenly ceased. As we were

then using the same water that we had been drinking for three months previously, and from the time of leaving England, there could have been no contamination of the water in this instance ; independently of the fact, that it was contained in tanks into which extraneous matter could not possibly have entered. A precisely similar outbreak occurred on board H.M.S. *Undaunted* while proceeding down the China Sea. As the cases continued to increase, the surgeon, at the end of three days, recommended the captain to change the course of the vessel. This was no sooner done, than the attacks ceased, not a case occurred afterwards. Again, at St. Thomas, in the West Indies, the inhabitants, in consequence of the water in the wells being brackish, drink solely rain water, which being stored in closed reservoirs and separately, in each house, contamination from external agents is utterly impossible. Yet the epidemic committed the same ravages in this island as in others. Surely therefore, the cause of cholera, is not in water, but in the air, and the conclusions at which we ought to arrive from a consideration of the preceding facts are, that the bacilli found in the bodies of cholera patients are the effects, not the cause of the disease. As bactarian parasites exist almost constantly in the intestines, and the comma-bacillus not more than any other, its presence can only be regarded as a coincidence not as a cause. Even Dr. Koch remarks : " From the coincidence of the latter (the cholera) with the finding of bacilli in the mucous membrane of the intestines, we cannot conclude that the bacilli are the cause of cholera. It might be the very reverse . . . which of these assumptions is the right one —whether the operation of infection or whether the

invasion of bacteria is the primary cause—that can only be decided by trying to isolate the bacteria from the affected tissues, to propagate them artificially, and then, by inoculation experiments on animals, to reproduce the disease.” But this test has failed in the hands of other experimentalists, as well as in those of Dr. Koch, who states : “ Several experiments have been made on rabbits, guinea-pigs, dogs, cats, monkeys, mice, rats, &c., but always without result.” The French medical commission, appointed to investigate the mode of action of the cholera and its method of propagation, remark in their report : “ The stomachic or intestinal dejections, or the contents of the intestinal canal—this last full of virgule or comma microbes—may, after filtration, be injected with impunity, into the cellular tissue of the peritoneum, the windpipe, the intestines, the rectum, and even into the blood. Virgule microbes, taken from the intestines of a cholera patient, may be introduced into the intestines of a rabbit, and multiply there for more than eleven days, without producing any choleraic symptoms, and without the autopsy revealing anatomopathological lesions characteristic of cholera.” Similar results have been obtained by the English Medical Commission in India ; and Dr. Klein, a member of the commission, has swallowed a number of the microbes without any ill result.

Although it would seem to be superfluous to prolong this inquiry, there is another circumstance which is fatal to Dr. Koch’s theory, that may be mentioned. This is that all these forms of bacilli exist constantly, and to a greater or less extent in all situations, and in all climates. As such, if this theory were true, the cholera would be always present, instead of prevailing at fixed periods and

at long intervals. These organisms are also found to prevail, to a greater extent, in populous districts than in others. But it is precisely there, as will be hereafter shown, that the ravages of the epidemic cholera are the least. Coincidence is not cause. If, indeed, we are to carry this doctrine to its legitimate results, we must conclude, that the worms found on the dead bodies of animals are not the effect but the cause of their death. We do not argue thus in such cases, neither ought we to do so in analogous ones.

As no other attempt to account for the production of a contagious virus has been made, that is worthy of consideration, there is only one method to be pursued, viz. to discard theories altogether—throw them to the winds, together with the germs of contagion—and trust only to facts and to experience. We shall then find ourselves, not in the unknown and wide realms of fancy, but on *terra-firma*—on which we may rest with something like security. The only thing to be guarded against is to ascertain that the so-called facts be facts, not invented or perverted tales.

If we turn to the works of the contagionists, more especially to those of the middle ages—an epoch of darkness, ignorance, and superstition—we shall find the most extraordinary accounts of the propagation of plague and other diseases. To repeat these now, or to attempt to refute them, would be merely a waste of time. We may judge of the value of them by some of those that have been published in the present day. At one of the Meetings of the Academy of Sciences in Paris, in 1832, M. Moreau de Jonnes attributed, *on the authority of diplomatic documents* that had been sent to him, the outbreak of cholera in the town of Oranbourg expressly to the arrival of a caravan from

Kirguis. Baron Humboldt, who happened to be present, stated that, on the contrary, the disease appeared with great intensity in Oranbourg *three months previously* to the arrival of the caravan, at which time he was himself there! But for this accidental circumstance, this tale, like so many others, would have been handed down to posterity as a proof, "strong as holy writ," of the contagious nature of the epidemic cholera. Talk of the Arabian Nights' Tales, they are nothing compared to the tales and the romances of the contagionists! Again, Dr. Gaëtani-Bey stated, that there was not a single case of plague in Cairo in 1835 in any of the establishments, and, particularly in the barracks, placed in strict quarantine. But M. Aubert-Roche stated, in his Report, that he received into the hospital of Ras-el-Tin, of which he was the physician, at this very time, and from these very barracks, 300 plague patients! With such fabulous tales as these, it is not surprising that the doctrine of contagion should have prevailed so long, more especially as, being generally believed in, no one took the trouble to ascertain its truth.

It was not until the last visitation of the plague in France, in 1720, that a reaction took place, and that the truth of the doctrine of contagion was impugned. Chicayneau, Veruy, and Deidier—who has been styled the chief of the anti-contagionists—ridiculed the notion, and attempted to show its fallacy.

Still the general opinion, both of the profession and the public, continued in favour of contagion; and the French surgeons who accompanied the army to Egypt at the end of the last century, all concluded, with the exception of Assalini, that the plague was propagated by

contact with the sick. Thus matters remained until 1835, when a number of physicians from France, England, and Italy had occasion to observe the visitation of plague in Egypt, in that and subsequent years. Concluding, with the generality of persons, that the disease was strictly contagious, they changed their opinion after having come in contact with the plague, as did MM. Brayer and Cholet, who studied the disease previously at Constantinople in 1819, 1826, and 1834.

Such being the case, it will not only be interesting but important, in the highest degree, to study the facts which led to a change of opinion in the minds of these scientific observers. The first conclusion at which they arrived was, that contact with the sick did not produce an attack. Numerous instances have been adduced by these writers in proof of this conclusion: one or two examples will suffice on the present occasion.

Dr. Ibrahim, a physician at Cairo, states that he was called to see the wife of Hassan Pacha in 1841, whom he found labouring under all the true and severe symptoms of plague. She died at the end of thirty-one days. This lady had in her service twelve white and twelve black slaves, four eunuchs, and four pages. These were in constant communication with the patient, and with the rest of the household of the palace—a hundred persons in all—and yet not one was attacked.⁷

We are also informed by Dr. Delong, another physician at Cairo, that a girl five years of age, in the house of Saad-Pinto in the Jews' quarter, was attacked with plague in 1841:—"She was nursed constantly by her mother; she was surrounded by her brothers, her sisters,

⁷ Rapport adressé au Conseil de santé du Caire. 1841. Document 17.

and her cousins; and she was in contact with all the household. The young patient died, but all the family continued healthy." Another fact is related by the same writer:—"Two young Turks belonging to the Cadi, the chief Judge in Cairo, were attacked with plague about the same time, and were placed in the same room. The disease was of a severe form. All the numerous persons attached to the Palace went to visit them: the visitors took the patients' hands, consoled them, nursed them, and touched them without taking any precaution. The two patients died, one shortly before the other. No other case was observed in all the vast enclosure of the Palace of Justice."⁸

To the question, "Is the plague propagated by contagion?" M. le Dr. Clot-Bey answers:—"I repeat, it has been demonstrated to me by a profound study of the history of this disease, and especially by the numerous facts that I have myself observed, or which have been made known to me by all the medical men who have had to combat the plague in Egypt lately, that the plague is *never* propagated by contagion." And to another question, "Is contact with an infected person necessary for the production of plague?" the reply of this experienced physician is:—"The *contact* with an infected person not only is *not necessary* for the production of plague, but it is of itself absolutely *innocuous*, and is only productive of danger because it brings the individual near the focus of the disease,"⁹ or of the cause productive of it.

Mr. Laidlaw, surgeon to the European hospital at

⁸ *Ibid.* Document 19.

⁹ Réponse aux questions posées par le Ministère Anglais en 1839.

Alexandria, in answer to the question from the same authority, remarks:—"I have under my eyes numerous examples of persons in health who have been in direct contact with plague patients, and who, nevertheless, have not been attacked: when the disease enters the family all do not fall victims; the persons who attend the sick continue healthy and well; those who render the last duties to the dead are not affected. I have seen a daughter, in spite of the supposed danger, throw herself on the body of her mother and embrace it until it had been carried away. I have seen a father raise his dying son, covered with plague blotches, in his arms, and retain him there until he had died. I have often, very often, been witness of the indifference with which the Turks and the Arabs place themselves in contact with the sick, without, in consequence, being attacked with the disease."

Another proof of the non-communicability of plague is derived from the prevalence of what are termed sporadic (single, isolated) cases of plague. The last writer states, "that sporadic (or single) cases of plague are continually occurring in Egypt and in Turkey, but they do not frighten the inhabitants. Experience has shown, that they are not productive of any danger; they break out in the centre of a town or a village, and pursue their course, without any other attack being observed, in spite of the number of persons who are placed directly or indirectly in contact with the disease. The natives have given the name of *kassif* (mild) to this form of plague, not because the symptoms are less severe, but because the disease does not become general. From the month of June, 1835, to the end of December, 1838, 649 sporadic cases of plague were observed in Alexandria; but the disease was not transmitted

to any of the persons who surrounded these patients and who attended upon them. The same fact had been observed before and commented on by Pugnet, at the commencement of the present century. If, also, we turn to the Bills of Mortality in London, a similar result will be apparent. Although the plague only prevailed epidemically in certain years, there are very few in which deaths from this disease are not recorded, although the number was generally small, sometimes only three or four. So, again, although the plague has never returned in an epidemic form since 1665, there were 1998 deaths in the following year, 35 in 1667, and 14 in 1668. After this the number never exceeded five, the last mention made of the plague being in 1679.¹ The non-propagation of the disease in the latter years, could not be referred, as Mead truly remarks, to the want of subjects, as only about a fourth part of the population had been cut off in 1665, while the places of these had been supplied by recruits from the country.

One more example will suffice for this part of the subject. There is a medical school in Egypt at Abouzabel, situated about four leagues from Cairo. When the plague

¹ It is often asserted that the cessation of the plague in London was the effect of the great fire that then occurred. This is an error. The plague ceased in February, 1666, but the fire did not occur until September. That the fire had nothing to do with the cessation of the plague, may be concluded from the fact that this disease disappeared at the same time in all other parts of England, although twenty or more towns were attacked the same year. It also disappeared at the same period in Paris and other places on the Continent. Added to this, there had been two great fires in London previously, in 962 and 1087; but they did not then cause a cessation of the plague. In the latter year, "St. Paul's Minster and the Bishop's Palace, together with many *other* monasteries, and the greater and handsomer part of the whole city of London, were burned down."—Anglo-Saxon Chronicle.

broke out in the latter town, the patients in the hospital and the hospital staff were sent out of the town and placed under tents pitched in the adjoining desert. They were attended by the professors and pupils of this school. "Five physicians," remarks Dr. Duvigneau, "of whom I was one, were attached to the hospital; each day we made two or three visits to the plague patients (sent there from Cairo), and autopsies were made soon after death. We never took any other precaution than that of washing the hands with vinegar and water or soap; we did not change our clothes before entering the hospital, and we approached and touched the patients the same as at other times. Well, in spite of all these means of propagation, not one of us experienced the least accident: not a single patient in the hospital (suffering from other diseases and attended by the same medical men), was attacked with plague; and yet the number of those who were treated, during the months of April, May, and June (and attacked with plague), amounted to a thousand."² These results are not new; similar facts have been recorded from the first appearance of the plague in Europe. Procopius states that, in the plague of 543, no physician or attendant caught the disease. Evagrius makes the same remark, and adds, that some, in despair for the loss of friends, threw themselves in the way of infection, and yet escaped; while others contracted the disease in the open market, and without communication with the sick. Lord Verulam remarks: "The plague is not easily received by those that continually attend the sick, as physicians; nor again by old people, and such as are of a dry cold complexion. On the other hand, the plague soonest seizes those that

² Reponse aux questions posées par le Ministère Anglais.

come out of fresh air, and those that are fasting, and children." Lastly; it has been stated by Dr. Fischer, that "several physicians of Cairo and Alexandria, who never went near a patient, but kept at a respectful distance, and who went out *enveloped in wax cloth*, mounted on horses whose bridles were made of cords from date-trees, and the saddles covered with straw, &c., were carried off by the plague."

In addition to the above, it may be remarked, that isolation—complete isolation—is not a preventive of plague. Of this fact numerous examples could be adduced, not only lately, but formerly.

In the *Recueil des pièces historiques sur la peste de Marseille* of 1720, published in 1820, we find the following: "That which appears the most strange is, that the plague attacked all those who shut themselves up in their houses the most closely, and who were the most careful not to receive anything, excepting with the greatest precautions. The plague insinuated itself there, no one knows how." A similar result was observed at Toulon. "The plague," said Antrechau, "seems to have been introduced into the Hôtel de Ville, merely to inform us that no barrier is able to arrest it. Neither our care to prevent all communication, nor our barriers, could preserve from its attacks those whom at first it appeared to spare. It found victims in dwellings, the entrance to which they thought was inaccessible, such great precautions having been taken to shut it (the plague) out." Similar facts were noted in Egypt. Dr. Dulany states, that the young wife of a writer named *Sgnerous*, chief clerk in the office of the Governor of Cairo, alarmed at the appearance of the plague, put herself into quarantine,

together with a few domestics. "She had no communication with her husband, who remained at his post, nor yet with any of the household. She contracted the plague nevertheless. Following my exhortations and example," continues the narrator, "they approached the patient; they touched and they nursed her, the most tender care being bestowed on her by her husband. She died at the end of five days. The disease was not transmitted to a single person, not even to an infant which the patient suckled, and which remained near her until the last moment."³ Again, "On the first appearance of the plague at Cairo," remarks Dr. Euzieres, "M. Antoum, wood merchant, shut himself up with his wife, four children, and several domestics. All the windows and other openings of his apartment were carefully closed, and the *cats* had been driven away! M. Antoum had performed a strict quarantine for about a month, when, on the 20th April, 1841, he was attacked with plague. I was called to see him on the 23rd. I found him in bed with one of his children of tender age; his wife was lying by his side on a mattress; the other three infants, as also the servants, were near the bed of the patient." He died on the 24th, eighteen hours after M. Euzieres' first visit. "Not a person in the house," adds the writer, "experienced the least symptom of the disease."⁴ Not only is contact with living and diseased or infected bodies thus shown to be innocuous, but contact with the same bodies after death is not attended with more danger. M. Seisson states, that he was witness to the cessation of the plague at Kauka at the very moment when the cemetery,

³ Loc. cit.

⁴ Rapport adressé au Conseil de Santé du Caire sur la Peste de 1841.

being overcharged with the bodies of the victims, gave out putrid exhalations in their greatest intensity.⁵ Clot-Bey, also, from having witnessed similar results, considers that putrid exhalations, or those from dead bodies, do not increase the ravages of the plague. This conclusion is confirmed by another fact. When the plague ceased in Moscow, the houses of all those who had died were fumigated, and were then taken possession of by their original occupants. Soon after it was discovered that the inhabitants, fearing to be sent into quarantine, had not made all the deaths known; they had concealed the bodies in their houses! These were found *in thousands*. They were interred, and not a single case of plague was observed afterwards.⁶ We also know that “the carriers of the bodies of those destroyed by the plague of London were peculiarly exempt from the attacks of the disease; the grave and pit diggers enjoyed a similar immunity. Butchers were also comparatively free from the disease; in short, all who were exposed to *putrid* exhalations seemed to find in them a *mighty antidote* against the in-odorous miasms which desolated the city.”⁷

With these remarks we may now pass on to a consideration of the next question—viz. Can the plague be produced or propagated by means of the secretions, excretions, or other morbid matters of plague patients?

“During the five months that the epidemic of 1835 lasted,” remarks Clot-Bey, “MM. Gaëtani, Lacheze, Bulard, and I, at Cairo; MM. Duvigneau, Seisson,

⁵ De la Peste, p. 208.

⁶ Schurrer: Faits réunis pour servir à l'histoire des maladies épidémiques, p. 63.

⁷ *Lancet*, July 11, 1831.

Perron, and Fischer, at Abouz-Abell; and MM. Rigaud and Aubert, at Alexandria, have attended plague patients in the hospitals and in private houses. Not one of us has employed any prophylactic or precaution. We were placed in immediate contact with plague patients, and at all periods of the disease. We received on our clothes, and on our hands, the vomited matter, the blood, and the pus, from *thousands* of buboes that we opened. More than a hundred autopsies were made at Cairo, and we passed entire hours in seeking, in the bodies of those who had died, those pathological alterations which had been so little attended to before. Dr. Rigaud is the only one among us who fell a victim to the reigning epidemic. But," adds the writer, "by a remarkable singularity, many physicians, who had scrupulously avoided the contact of patients and of suspected objects, were attacked with the disease and died. Of this number were Drs. Manucci (senior), Leopold, and Lardoni."

Not only was the mere contact of diseased secretions and morbid productions unattended with danger, but the introduction of these substances into the blood was not followed by any fatal result. In 1803, M. Valli, an Italian physician, inoculated twenty-four persons with a mixture of variolous and plague matter, in order to test the value of some fanciful speculations of his own—viz. that the presence of the variolous poison would prevent the operation of that productive of the plague. Dr. Sola, also, a Spanish physician, inoculated fourteen deserters condemned to death, in 1818, at Tangiers, with a mixture of plague matter, or pus, and oil, from an idea that the latter would prove an antidote to the former. Be this as it may, neither in this instance, nor yet in

the former, were those inoculated attacked with plague. That the variolous matter and the oil exerted little influence in warding off an attack of plague, may be inferred from the following experiments. They were made in 1835, at the Hospital Esbékîè, in Cairo, in the presence of Gaëtani Bey, Clot-Bey, Dr. Lacheze, and Dr. Bulard:—

Three men, condemned to death, were voluntarily subjected to the following proofs, a free pardon having been first granted them. A lancet, charged with *the blood* of an infected patient, was introduced under the skin on the inside of the arm, on the 18th of April. One of these was attacked with plague on the 21st April, but in a mild form, as he was convalescent on the 26th. The other two experienced no ill effect. A fourth, who had experienced a slight attack of plague on the 15th April, was inoculated on the 5th May, in the groin and under the armpit, with the serosity taken from the carbuncle of a plague patient, and eight days after with the blood of another patient; but no ill result followed either operation. We thus find that one man out of the four was attacked with plague; but the wonder is that they were not all attacked, having been brought from the gaol, where the disease did not prevail severely, into the very focus of the epidemic. The escape of the other three shows very clearly, that the blood of an infected patient fails to produce plague when introduced into the system of another and a healthy person. With the view of confirming the truth of this conclusion, Clot-Bey inoculated himself with *the blood* of a plague patient in six places—three in the left forearm, and three in the right groin. No effect followed!

In addition to the preceding, a fifth criminal was inoculated, on the 20th April, with the serosity taken

from a carbuncle, but this not producing any effect, he was again inoculated, on the 30th, both in the groin and in the armpit, with some pus taken from a bubo that had just been opened. No attack of plague, or other ill effect followed the operation.⁸ Dr. Clot-Bey also inoculated himself on the inside of the left arm with some pus taken from the bubo of a patient. Slight *malaise*, and other symptoms, having no analogy with plague, were alone experienced.

From the above facts we shall be justified in concluding that the blood, the secretions, and the morbid productions of plague patients, do not produce an attack of the disease, when introduced into the body of a healthy person. There are, as might be expected, other examples in which attacks of plague have followed inoculation with pus and other matters; but, then, the operation was performed within the pestilential area. This was the case with Mr. White, an English surgeon, who, according to the Report of Sir James M'Gregor, inoculated himself with pus taken from the bubo of a plague patient, during the stay of the British forces in Egypt. He died a few days after, a result that might have occurred precisely the same had he not been inoculated. Had the experiment been made beyond the morbid boundary—for these diseases invariably have a well-defined boundary—a different conclusion would, of course, have to be drawn. But there is no example of such an experiment having been made.

As already mentioned, not only are animate bodies considered to be the vehicle for the propagation of the

⁸ Communication faite à la Commission de l'Académie de Médecine, par M. le Dr. Lachèze. Document No. 25.

plague, by the absorption and extrication of the virus of contagion, but inanimate objects, also, are said to possess the same property. This conclusion, which is an important one, will require almost as attentive a consideration as the preceding question. The articles that have been considered most liable to become infected are the clothes and the bedding. The former would be quite as dangerous as the latter, in consequence of the practice which exists in Egypt and Turkey of not changing the clothes at night. More than this, patients not only wear their ordinary clothes during illness, but the majority also die in them. Notwithstanding, the clothes of plague patients in Turkey and in Egypt are not destroyed.

We are informed by Dr. Brayer, that the Jews of Constantinople have magazines for the clothes worn by the Mussulmen and the *rayas*. "If the plague rages, the market is crammed with clothes. It was there that the effects of 150,000 victims to the epidemic of 1812 were collected. Do not suppose," he remarks, "that they trouble themselves to disinfect them: no one dreams of such a thing. A part of these clothes passed quickly into the hands of the inhabitants of Constantinople: another part was sent into the principal towns of Turkey. What remained unsold was heaped up in the magazine—small, dirty, obscure, without windows, and where the air could not circulate. These were re-sold the following year. Notwithstanding, although the cases of plague were sufficiently numerous at the commencement of the month they ceased entirely at the end of December."⁹ It is worthy of remark, that the Jews, who deal in, and live among, these infected clothes, suffer less from the plague,

⁹ Neuf Années à Constantinople, t. 2, p. 354.

according to this writer, than the Greeks, who are contagionists, and take every precaution to prevent the disease. The observations made in Egypt, and the facts collected, during the recent visitations there, are still more conclusive. After the cessation of plague at Cairo, in 1835, all the furniture and the clothes of those that had died were sold in the bazaars; the latter being worn afterwards, although not disinfected. The effects of more than 50,000 plague patients, who had died in this capital, did not, says Clot-Bey, communicate the disease to a single person.¹

The same writer also states, that the hospital of Esbequié, at Cairo, received more than 3000 plague patients in 1835. When the epidemic ceased, the hospital was restored to its former destination, and received again those who were suffering from ordinary disease; and this, too, while convalescents from the plague were still there. "They were placed in *the very beds* in which plague patients had died. The sheets only were changed. They gave them woollen coverlids, which had not been disinfected, which had not even been aired, since they were used by plague patients. Well, more than 500 of these coverlids, yet *impregnated and saturated*, as we may say, with the emanations of the infected, and a multitude of other objects, which had been placed at their disposal, did not give the disease to a single person." "The fact is," remarks Dr. Seisson, Professor of Pathology in the Medical School, at Cairo, "the plague ceases here at the end of June, at the moment when Cairo contains a multitude of objects of every kind, which had belonged to the victims of the epidemic: and when a great part of the

¹ Rapport de l'Académie de Médecine, p. 104.

population is clothed in the garments of the dead ; for as the Arab does not take off his clothes, when he goes to bed, the greater number of the patients passed their illness and died in the same clothes. In the hospital, also, the clothes of the patients, who have died of plague, are placed indiscriminately with those taken from other bodies.”¹

We are also indebted to Clot-Bey for the following particulars, which tend to prove, that the plague cannot be propagated from country to country by means of infected articles of clothing. “The Egyptian army, which then occupied the country, suffered severely from the plague which raged in the Morea in 1826, 1827, and 1828. In September of the last year, the troops returned to Egypt, and the clothes of all the soldiers who had died, both of the plague and of other diseases, were taken to Alexandria, deposited in the magazine of a barrack, and eventually sold. Nevertheless, not a single case of plague was observed that year in Egypt.”²

Similar results had been observed and recorded previously in Europe. “One thing,” said Francis Poona, “which was observed and is worthy of admiration, is, that among so many *employés*, and so many men vulgarly called *purificators*, who handle every moment these same clothes on which plague patients had lain and died, *not one was infected by it*—a result contrary to so many opinions, and so many conclusions drawn by the mind of man, and which induces us to say, with Hippocrates, that there is, in diseases, a something, we know not what, of

¹ Réponse aux sept Questions posées par le Ministère Anglais.

² De la Peste.

Divine.”³ Another and a more recent example has been recorded. In 1713, there was a visitation of the plague, in Holstein, shortly before the Swedish troops entered the province. “No precaution,” as we are informed by Schurrer, “was used to preserve them from the disease, yet no one was attacked, although they *wore the clothes and used the beds of the plague patients*.”⁴

With these facts before us—not imaginary tales, but well-authenticated facts—it seems to be impossible to draw any other conclusion than this: that the clothes worn by plague patients do not contain the germs of the disease. They cannot, therefore, give the disease to other and healthy persons, even when worn by them. And yet tales are told of the plague having broken out after boxes and trunks, coming from an infected locality, have been opened. But all these instances have occurred within the epidemic radius, and may therefore be referred to other causes—to that which produces the disease in other instances. There is no case on record of the plague having broken out after such an occurrence beyond the epidemic area. As the French Commission, in their Report, remark:—“Facts in great number prove, that the clothes and other articles which have been used by plague patients have not communicated the disease to those persons who have worn them, although not previously purified. The facts, which would appear to give the opposite result, cannot be of any value unless confirmed by new observations made beyond the epidemic focus—far from the centres of miasmatic infection,

³ Histoire de la grand Contagion de Verone, en 1630, p. 103.

⁴ Loc. cit., p. 61.

and far from the countries where the plague is endemic.”⁵

If the clothes and other articles, worn by patients, and saturated with the morbid secretions, do not give the plague, it is improbable that articles of merchandise, handled by the healthy only, can produce a different result. Numerous instances are recorded, nevertheless, of the presumed transmission of the plague by merchandise; but, then, the majority of these cases, if not all, occurred during epidemic periods and within the morbid boundaries of the disease. Hence, some particular occurrence which was observed at the time, but which would otherwise have escaped notice, has been set down as the cause of the outbreak. Thus Hodges states, that the plague of 1665, in England, was introduced by the importation of some bales of cotton from Holland. But the plague prevailed in London, and sporadically, the previous year: while this accidental circumstance will not account for all the previous visitations in England—twenty-two in number. Then, again, if a few bales of cotton could spread the plague around, how was it that the thirty-five persons who died in 1667, and the fourteen in 1668, as also those in subsequent years, did not produce the same result? The fact is, that when a particular phenomenon arises, the cause of which is unknown, men are apt to ascribe it to the first accidental and coincident circumstance that strikes their imagination. Thus, it was stated and firmly believed, that the last plague at Marseilles, in 1720, was imported by a vessel which had arrived at the time from Syria. But it was proved, subsequently, by a commission of medical

⁵ Loc. cit., p. 112.

men, sent there by the French Government, that several persons had been attacked with the true symptoms of plague, before the arrival of this vessel.⁶ Other and similar accounts, if investigated, would, there can be little doubt, prove to be as fabulous as this. "It is commonly said," remarks Assalini, "that on opening a letter, or a bale of cotton, containing the germs of plague, men have been thrown down and killed by the pestilential vapour. I could never meet," he adds with much *naïveté*, "with an actual witness of the fact, in spite of the researches that I have made in the Lazarets of Marseilles, of Toulon, of Genoa, of Livourne, and of Malta, and in the Levant; all agree that they have heard of such things, but that they never saw them. Among these was the captain of the Lazaret at Marseilles, who said to me, he had seen *millions of bales* of cotton, silk, and wool, skins, feathers, and other articles, coming from places in which the plague existed, opened *without* his ever having observed any accident to arise therefrom." It could hardly have been otherwise, for no ill result has followed, during the present century at least, from the immense quantity of merchandise that has been imported into Europe from Egypt and other places; not only in

⁶ In a letter published by Dr. Deidier, Professor of the Faculty of Medicine in 1721, and sent to Marseilles by order of the King, the certificates of Drs. Robert and Rimbaud are inserted. They state, that the ship of Capt. Chaseau did not arrive at Marseilles until the 25th of May: whereas Madl. Angier died of a disease with the characteristic symptoms of plague, on the 20th of April: and Madl. Coursand on the 4th of May; while a female, named Rose, was also attacked on the 20th. Added to this, the ship was placed in quarantine, and none of the passengers, or goods, were admitted into the town until the 14th of June, when the disease had become general.—*Journal des Savans*, pour l'Anné 1722, p. 611.

the intervals but also during the prevalence of the plague. To show this, one example will suffice.

During the severe visitation of plague in Egypt, in 1835, cotton continued to be exported during the whole period of its prevalence, and, even, at the very height of the epidemic. According to Dr. Laidlaw, in a Memoir addressed to the English Consul-General at Cairo, there were exported, that year, from Cairo:—To England, 31,709 bales of cotton; to France, 33,812; to Trieste, 32,262; and to Holland, 150. But no instance is known of plague having appeared in any of the ports or countries to which these ships were consigned; although no steps were taken to disinfect the cotton. Of the sixteen English vessels, charged with cotton, eight had plague patients on board; but this circumstance made no difference in the result. It may also be added, that none of the labourers employed at the Lazaret at Marseilles to unload the vessels, have contracted the plague since 1720—the year of the last visitation there. That they should have been attacked previously is not surprising, as these men were then living within the epidemic focus, and liable, therefore, to be attacked in common with other persons, from the operation of the morbid cause, whatever that may be.

There is another circumstance that may be mentioned which shows very clearly, that the plague cannot be propagated by either animate or inanimate objects. This is, that although there are neither Lazarets, nor quarantine, the plague never spreads beyond certain well-defined boundaries to the south and east of Egypt. Thus, it is unknown in Upper Egypt beyond Assuan, or the first Cataract; in Nubia, Abyssinia, Sennar, and all the coun-

tries to the south of this line. "Here, then," exclaims Dr. Aubert, "is a country in which each year 70,000 or 80,000 persons, coming from localities in which the plague reigns, are assembled, without spreading the disease, although they carry merchandise and other articles with them. So also if patients, infected with plague, are carried to Philoe, beyond the morbidic boundary, they die or recover, without ever giving the disease to the inhabitants." ⁷ Its limits eastward are equally well defined. According to Dr. Duchèze, the plague never passes beyond a line drawn from the Caspian Sea to the mouth of the Persian Gulf. Hence it has never appeared at Teheran and Ispahan, although it carried off at Bagdad, in 1831, half the population. Even here, remarks the above writer, it spared certain quarters of the town, notwithstanding the constant communications with the infected localities. Dr. Duchèze also states, that *the intervals* of the return of the disease *are longer*, the farther you proceed towards the south-east, from Trebizond to Aleppo. These are remarkable facts, and entirely opposed to all the deductions drawn from the doctrine of contagion. Were plague contagious, in the slightest degree, it would be, we should conclude, precisely in southern latitudes—in warm climates, and among half-civilized nations—that the disease would spread with the greatest ease, and with the greatest rapidity. And yet, it is precisely in such situations, where no barriers are erected, and where no *cordon sanitaire* has been formed, that nature draws an invisible line, and exclaims, "Thus far shalt thou go, and no farther." When, however, man erects a barrier, and establishes a

⁷ De la Peste, p. 101.

cordon sanitaire, she laughs, to very scorn, his puny efforts to arrest her progress. Thus, the epidemic cholera, during its first visitation in Europe, marched directly across a *triple line* of bayonets, formed with 50,000 Prussian troops—a *cordon* so rigidly kept, that even a cat, it was said, could not have passed across. The same result is observed, sometimes, with respect to elevation. On the mountain of Alemtaghe, five leagues from Constantinople, there is a village, which has hitherto remained exempt from the plague; although it has always served as a place of refuge to the inhabitants of the infected city. This is the more singular, as another village, half way up the mountain, does not enjoy the same immunity. This singular fact, the non-propagation of the plague by infected persons, beyond the epidemic area, has been observed and recorded long ago. Evagrius, speaking of the plague of the sixth century, remarks: “But what above all appeared singular and surprising was, that the inhabitants of infected towns, removing to places where the disease had not appeared, or did not prevail, were the only persons who fell victims to the plague, in the cities which *were not affected*.”^s

Although it may be superfluous to do so, there is yet another argument that can be employed, in order to show, that contagion has nothing to do with the propagation of the plague. This is, that the disease invariably subsides in Egypt at a particular and certain period. When the plague prevails epidemically in that country, it commences in the month of November, and ceases its ravages the end of June. This fact was mentioned, long since, by Prosper Alpinus, and has been confirmed by nearly all

^s Hist. Eccles. lib. 4, ca. 29.

writers since. Hence, as Dr. Lachéze remarks: "When the 25th June arrives, the ordinary period for the cessation of plague in Egypt, all precautionary measures are abandoned; and yet, no ill results are known to follow. This must not be ascribed to the fact, that all the susceptible people have been attacked; for strangers, who then arrive in considerable numbers, also remain exempt. The negroes of Sennar are brought into the market to replace the black slaves that have died; and are generally clothed with the garments worn by the plague patients. In 1835, out of 600 negroes brought to Cairo, in this way, fourteen only were carried off." ⁹ This result cannot be ascribed to season, or temperature, for although the plague appears at the commencement of winter, and disappears at the beginning of summer in Egypt, the reverse is the case at Constantinople. Here, the plague usually commences in July—from the 1st to the 20th—and ceases on the approach of winter; so that, as one writer has remarked, while 16 degs. of Réaumur are sufficient for its production in Egypt, 25 or 30 degs. are required in Constantinople. This *invariable* cessation of the disease, at a particular epoch, is fatal to the doctrine of contagion, more especially as, the fact being well known, the means previously adopted to prevent the spread of the disease are then laid aside. "What," adds Dr. Perron, the Director of the Medical School at Cairo, "is the contagion of a disease, which ceases *officially* at a particular season; at a particular period of the season; at such and such a temperature of the air; and which varies, in its intensity of action with the variations of the atmosphere

⁹ Séance de l'Académie Royale de Médecine, du 28 Décembre, 1844.

—of humidity, of cold, of the sudden transitions of the day and night. If this be called contagion, then we have nothing now to do with that which is called the contagion of men and things.”

There is another circumstance that is well worthy of consideration. As has been already stated, sporadic cases of plague are met with constantly in Egypt, in years in which the disease does not prevail epidemically. These single and isolated cases,—varying from one to ten, twenty, thirty or more,—must arise spontaneously, as there is no other way in which to account for their production. Mr. Laidlaw states, that he visited, in the port of Alexandria, a sailor, recently arrived from England, who had been attacked with plague, although the disease did not exist in the town, or elsewhere. As such, he could not have contracted the disease from any one. Nor, on the other hand, did he infect the rest of the crew, although he slept in the midst of his companions for several nights. In other instances, the disease will spring up spontaneously in a town without extending beyond. Baron Larrey states, as he learnt from the inhabitants of Jaffa, that the plague appeared there every year; and had done so for thirty years previously; although the disease did not prevail elsewhere, or in any other town. If, therefore, plague can arise spontaneously in one locality, it can do so in another, provided only that the causes productive of the disease be in operation in that locality.

These are anomalies that can never be explained by a reference to the doctrine of contagion; they only admit of explanation on one supposition, viz. that plague is merely an aggravated form of ordinary fever. Sydenham, Lieuland, and other writers have compared plague

to a malignant fever. It is not, in fact, the only disease in which swelling of the glands, carbuncle and petechial spots, are observed. Hence it is, that the plague has almost invariably been ushered in by malignant, or severe, forms of fever: while the latter have almost as generally taken the place of the former, on the termination of the epidemic. "It is observable," says Dr. Heberden, "that, at its first breaking out, the disease is never known to be plague. It has generally been preceded by a severe putrid fever. This was the case in London, in 1665; at Marseilles, in 1720; in Holland, in 1764, and at Moscow, in 1771." In the last city, fever prevailed for three years, before the plague broke out. A severe or malignant form of fever was also observed at Algiers in 1816-17. This was regarded as the *avant courier* of the plague, a prediction that was realized the following year. Sydenham has remarked, that he did not then know, if the disease, which broke out in May, and which afterwards merged into the plague, was the plague or not. More than this, since the plague has ceased to prevail epidemically in Europe, it has been replaced by another, and a common, form of fever, viz. typhus; which is merely a mild form of plague—the one disease frequently running into the other. Dr. Pruner states, that the plague of 1835, was preceded by malignant fevers of a peculiar character: as, also, by a visitation of smallpox. It was followed by the cholera and typhus, well marked cases of plague occurring at the same time as the latter.¹ When typhus appears in a severe form, as

¹ Answer to Questions proposed by the English Consul-General in Egypt.

is sometimes the case, it is scarcely possible to distinguish it from plague.

This is not all. In particular instances, and in certain localities, the plague would appear to be merely a severe form of intermittent fever. This was rendered very evident during a campaign of the Russians in Turkey. On arriving at Bucharest, in April, 1828, the Russian troops were attacked with a fever, which was accompanied by vomiting, great debility, and, with some patients, by buboes and carbuncles—being followed generally by death at the end of three or four days.

The medical men of the town stated, that such a disease was common there, and that it usually subsided at the commencement of the hot weather. This was found to be the case; but the disappearance of the disease was only temporary; it re-appeared in August, and became more general than before. At this period, however, the plague was preceded, in a great many places, by a general prevalence of remittent and intermittent fever. And it is added, “these diseases were productive of greater ravages, during the whole course of the campaign, than the plague itself.”² Precisely the same results were observed the following year, except that the disease was more general and more fatal—especially at Varna, where its ravages were terrible. In July, the army crossed the Balkan and occupied Ardos, when intermittent fevers began to prevail. These continued until August and September—the hospitals at Ardos and Adrianople, all this time, being crowded with cases of intermittent fever and dysentery. “It was not until

² Extrait des Notices de M. le Dr. Seidlitz : par M. le Dr. Bussemaker, d'Amsterdam.

the 19th October, after peace had been signed with the Turks, that the first case of fever, accompanied by a bubo and a carbuncle, made its appearance. These cases soon began to multiply, and to present the aspect of a well-marked epidemic of plague: it committed, consequently, frightful ravages at Adrianople,"³ where the principal part of the army was stationed.

"But," observes the same writer, in another place, "it is not only while considering these epidemics in their entirety, and, as a general fact which induces us to infer, that the plague, during this war, was only the last development of the endemic intermittent fever: the study of particular cases of these diseases appeared, sometimes, to confirm entirely this opinion. Thus Dr. Milovanof, who treated the plague at Achial, tells us: 'The soldiers and officers who had intermittent fever, suffered also with buboes and carbuncles: from the first establishment of the hospital, we have observed tumours and swollen glands at the neck. In the month of September, the disease was met with, principally, in the relapses from intermittent fever, while it took the form of a tertian ague.' At Adrianople, also, the first soldier who was attacked with plague, had been in the hospital for two months suffering from intermittent fever." Dr. Rink, who was stationed in this town all the time of the epidemic, remarks: "The mildest form of plague resembles intermittent fever so much, that it was almost impossible to distinguish the disease, before the appearance of buboes." And he adds: "It is to be remarked, that this form of plague was more common at the commencement and towards the end of the epidemic than at its height."

³ *Idem.*

Hence,—and also from the fact that, whenever the Russians have invaded Turkey, the plague has made its appearance on the borders of the Black Sea,—Dr. Seidlitz has enunciated the opinion, that “the plague, on these occasions, is only the severest form of the endemic fevers of the country.”⁴

Dr. Begin⁵ and Dr. Boudin⁶ have also inferred that plague belongs to the family of marsh, or malarious fevers. If so, we can understand, why intermittent fevers were so common in Europe during the prevalence of the plague, of which we had abundant proof even in London. In 1661-2-3 and 4, intermittents raged like a plague, to use the common expression, and ushered in the true plague of 1665 and '66. These fevers were also accompanied by a severe continued fever. They then disappeared for some years, for reasons, as Sydenham remarks, with which we are at present unacquainted. After a short interval, these fevers returned, and continued to prevail epidemically until the middle of the eighteenth century, when a severe visitation was experienced. They then gradually subsided in London, and had almost entirely disappeared at the commencement of this century. With these facts before us, there can be no difficulty in accounting for the spontaneous outbreak of plague in any locality in which intermittent and remittent fevers are endemic, and in which the plague formerly prevailed in an epidemic form. This is not the only conclusion we can draw on the subject. If plague be only a severe form of

⁴ Med. pract. Abhandlung. 1835. Also, Memoires sur les Sciences Médicales, publié par l'Académie Imperiale de S. Petersburg. 1844.

⁵ Dictionnaire des Sciences Médicales. Art. Marais.

⁶ Géographie Médicale, p. 43. Paris. 1843.

intermittent or remittent fever, and if it be produced by the same cause—as the preceding facts oblige us to conclude—we must also infer, that, plague irrespective of all previous arguments and conclusions, is non-contagious. Not only intermittent and remittent fevers, but those severe forms of continued fever, met with in tropical climates, are universally allowed not to be contagious—the facts presented during their prevalence forbidding such an inference. The examples, before given, of the attacks of fever with sailors, who landed in pestiferous localities; and the exemption of their messmates among whom they were placed, in the confined deck of a man-of-war, are sufficient to prove the truth of this conclusion. If, therefore, a disease, presenting the same pathognomonic symptoms, with some slight variations, and which apparently is produced by the same cause, be non-infectious; we cannot err much in concluding, that plague also is incapable of propagation from individual to individual. This conclusion is in accordance with all the facts previously advanced, and with which we are acquainted. Of what use then are Lazarets, quarantine, and all the obstructions and annoyances to commercial traffic and human intercourse? They can be of no earthly use, or benefit whatever: while they are productive of direct injury, by producing the very result they are intended to avoid—that is, outbreaks of disease.

The truth of these conclusions has been demonstrated, in a satisfactory manner, by the researches of Dr. Rossi, who instituted a series of inquiries respecting the practical results of the system. His object was to ascertain, if Lazarets and quarantine prevented the appearance of the plague, in those countries in which they had been esta-

blished ; and the result—the unexpected result—of his researches shows, that the outbreaks of plague were actually more frequent after than before the establishment of Lazarets, at least in Europe. The case is different with Turkey and Egypt, Lazarets having only been recently established in these countries. These facts will be evident by a reference to the following Table, in which the number of outbreaks of plague in each country, before and after the establishment of Lazarets, has been inserted.⁷

By the study of this Table—Table 2—some remarkable facts may be elicited. Thus, in France, there had been, from the commencement of the Christian era to the establishment of Lazarets, 29 visitations of plague ; being 1 every 50 years : while there were, subsequently, or from that date to the cessation of the plague, 27 visitations. This gives an average of 1 visitation every 9 years—a remarkable difference. The same result is apparent in all the other countries in which Lazarets were established, with the exception of England and Holland, in which no visitations occurred subsequently. The reason is that the plague had ceased in England 55 years before the institution of Lazarets ; and in Holland, 31 years previously. That the formation of the Lazarets had nothing to do with the cessation of the disease in England is certain, from the fact that the plague subsided, at the same time, in the north of France and other countries in the north of Europe. It may also be stated, that although there had been no visitation of plague for 55 years before the formation of Lazarets, there were no less than 6 in the previous 65 years.

It may however be asserted, that the plague had been

⁷ See Table.

introduced into the preceding countries—those in which the plague appeared after the establishment of Lazarets—by land, instead of by sea. But this supposition does not apply to Venice, which cannot be approached by land. Hence, Dr. Rossi was careful to ascertain the exact number of visitations of plague that this city has experienced. There were, in the 465 years before the establishment of Lazarets and quarantine, 4 visitations of plague in Venice; viz. in A.D. 938, 1006, 1347, and 1405: this gives an average of 1 in 116 years. In the 227 subsequent years, there were 16 irruptions of plague, viz. in 1411—13, 38, 47, 56, 64, 68, 85, and 96; also in 1500—23, 27, 56, and 1630. This is an average of one visitation in every 14 years! These statistical results confirm the deductions previously drawn, as to the uselessness of Lazarets, quarantine, and other measures, invented by foolish and ignorant men, to prevent the operation of natural laws. Vain effort: they may as well attempt to arrest the sun in his course, or the moon in her orbit, as try to prevent the irruption of epidemic diseases. It is also to be remembered, that Lazarets can only be useful on the supposition, that the plague is always imported by ships. But pestilences do not always commence on the sea-coast. That in the time of Thucydides began in Ethiopia, on the borders of Upper Egypt. The plague of A.D. 252 also began there, as did the one in 1736. So, again, the plague of 1348, which ravaged France, broke out at Avignon, not at Marseilles or other ports: as was the case, also, in 1482. But the most remarkable fact was the breaking out of the plague in Poland in the visitation of 1702: before any other place or country was infected!

These are not the only facts that are brought out by a study of the preceding Table. It is commonly supposed, and many arguments and conclusions are based on the supposition, that Egypt is the home, the birthplace of the plague. Nothing can be more erroneous than such a conclusion. Up to the end of the twelfth century, only two visitations of plague in Egypt have been recorded; and only six more to the end of the seventeenth century—eight in all. During this period, there had been twenty-two visitations in England, and fifty-five in France. After this the reverse is the fact; in the eighteenth century, when the plague had ceased entirely in England and other countries in Europe, there were no less than nineteen outbreaks in Egypt. This shows that the plague, so far from having been imported from Egypt into Europe, might be said, with more truth, to have been imported into Egypt. As will be remembered, the crusades to the East took place in the twelfth and thirteenth centuries: the very period when the plague raged to the greatest extent and the most frequently in Europe. And yet, during these two centuries, there was only *one* visitation of plague in Egypt; one in Syria, and not one in either European or Asiatic Turkey or Palestine. Again, the commercial relations of Europe with the East, by the Mediterranean, were the greatest at this epoch, and up to the end of the sixteenth century: after this, they were diverted into another channel, by the discovery of the Cape of Good Hope. Now it was precisely at this period, when commercial intercourse and traffic with the Mediterranean had almost ceased, that the plague began to prevail in Egypt more frequently, and to a greater extent; thus showing, that this disease pursues its own

course, irrespective of commercial traffic and human intercourse.

With such an amount of evidence, direct and indirect, we may now conclude this part of the subject, by drawing the following inferences. 1st. That the plague is not propagated by contagion, either mediate or immediate : in other words, by the contact, direct or indirect, of the healthy with the sick. 2ndly. That the clothes, bedding, &c., of plague patients, even when saturated with the secretions and excretions, do not give the disease to those who may handle or wear them afterwards. And 3rdly. That articles of merchandise do not imbibe the germs of plague, and cannot, consequently, convey the disease to other and distant countries.

If the plague be not contagious, we might also infer *à priori*, that no other disease would be. But this is not the general opinion : a number of other diseases—the majority, in fact, of epidemic diseases—are also considered to be contagious. One of these is yellow fever. After the discussion that has taken place, respecting contagion in general, it would be superfluous to enter into any detail of the alleged instances of the propagation of this disease by infection. It is sufficient now to remark, that all the circumstances and arguments which have been advanced, in support of the contagiousness of yellow fever, would appear to be negatived by the following facts.

1st. Yellow fever is confined to certain latitudes, and well-defined boundaries, beyond which it never passes. It is unknown beyond 45° N. latitude, and 23° S. latitude, and between 20° E. longitude, and 120° W. longitude. The chief seat of its ravages is between latitudes 10° and 30° N.—in the West India Islands, on

the continent of America, and on the West Coast of Africa. It is unknown in the interior, in the southern, and eastern parts of Africa, as also in the whole of Asia. It has prevailed on several occasions in the south of Europe; at Cadiz, Seville, Gibraltar, Malaga, and along the eastern coast of Spain, as far as Barcelona. It reached Leghorn, on the one side, and Portugal on the other; but it has never extended beyond these boundaries in Europe. From the Equator to the 26° or 28° N. the yellow fever may, and does, prevail all the year round; but from this line to the 43° N. only during the heats of summer. In the former latitudes, the inhabitants are rarely attacked, only strangers; but in the latter, or northern latitudes, the inhabitants are as subject to its attacks as strangers. How a disease, which is confined to a particular portion of the earth's surface, can be regarded as contagious, must be inexplicable to ordinary minds; more especially when no means are taken to prevent its spread. Yellow fever prevails, it is true, principally in large commercial towns, and seaports, but it is not confined to such situations. It spreads into the interior of America, as it did also to the small towns and villages near to the coast of Spain; although it did not extend beyond, in the latter situation. But there is no reason why it should not have done so, if the disease be contagious.

2ndly. Although the torrid zone would appear to be its source, yellow fever is not generated exclusively there, nor does it always spread from these to other and higher latitudes. Instead of commencing in the south, it occasionally pursues the opposite course. This was the case in 1798, when the epidemic commenced at Boston, in

lat. 45° , in June, and travelled southwards, reaching New York, in lat. 41° , in July. Its course, through this town, was also the same, having commenced at the northern extremity, and then proceeding in a southerly direction.

3rdly. Removal from the focus of the disease, or the locality in which it prevails, is always sufficient to arrest its progress. This is so well known, that men-of-war, on the West India Station, depart immediately, when yellow fever breaks out on board, and make for other and more northern latitudes. But the disease has never been carried to the North American colonies, nor does it spread to the remainder of the crew, excepting with those who have imbibed the seeds of the disease before their departure. That the subsequent attacks, when they occur, ought to be referred to this cause, we may learn from another circumstance. It sometimes happens, that a part of the crew only is exposed to the causes productive of yellow fever. When this is the case, the disease is never found to spread to the other men, although placed in immediate contact with them. In 1782, "the *Assistance* (man-of-war) wooded and watered at St. Thomas—a noted place for yellow fever—and, with a view to expedition, a tent was erected on shore, in which the people, employed on these services, lodged during the night. On the middle passage, *every man*, who had slept on shore, was attacked with fever and *died*: while the rest of the ship's company remained perfectly healthy."*

This immunity of high northern latitudes—those beyond the 45th degree—and the cessation of yellow fever,

* Trotter : *Medicina Nautica*, vol. i., p. 456.

when a ship arrives there, or near, cannot be referred to temperature or cold, the heat being sometimes as great in high northern latitudes, as in more southern ones. Besides, although the epidemic spreads as far as 43° , or 45° N. latitude, it does not pass beyond the 23rd degree, in the southern hemisphere, although the heat, in that latitude, is nearly tropical. Then, again, it is precisely in a northern direction that yellow fever spreads, when it becomes epidemic, passing from the West India Islands to the coast of America, and travelling from the south to the north of this continent.

Added to this, we observe precisely similar results in the same locality or latitude. One instance has been already given: that of soldiers on guard at the Dockyard, Antigua, being attacked and dying of yellow fever, while their comrades in the barracks above, to which they were removed, remained entirely exempt. The barracks here, it is true, are at some elevation, and, consequently, enjoy a cooler atmosphere. But, then, the same result is observed at Trinidad, where the circumstances are reversed. There, the military post, was on the La Ventille Hill; while the barracks are on a plain—on the same level as the marsh. Nevertheless, men attacked with fever on the hill, and taken to the barracks below, did not give the disease to others, in this situation, more than at Antigua. It has also been stated, that strangers are constantly attacked with yellow fever within the torrid zone, although the residents and natives remain exempt. Cold, therefore, cannot be the cause of the non-propagation of this disease in high northern latitudes. What the cause may be of the limitation of range of yellow fever, is immaterial at the present moment: it is sufficient now

to know, that this disease cannot be propagated, either within or without its natural boundaries, by means of contagion. The preceding facts are more than sufficient to prove the truth of this conclusion. Notwithstanding, one more proof may be added. This is that the attendants on the sick, in yellow fever, the same as in other diseases, are less prone to attacks than other classes. In the following Table, the proportion of attacks and deaths among the white troops, stationed at Newcastle, Jamaica, during a visitation of yellow fever, has been given; those who attended on the sick being separated from the others:—

TABLE III.

PROPORTION OF ATTACKS AND OF DEATHS, FROM YELLOW FEVER, AMONG THE FOLLOWING CLASSES.⁹

	Strength.	Attacked per cent.	Died per cent.	Percentage of deaths to attacks.
Men who attended fever cases	} 156	5.1	1.9	21
Men who did <i>not</i> attend on the sick	} 523	17.0	7.3	41

Not only was the ratio of attacks and of deaths, among the attendants, very much less than among other classes; but, what is still more singular, the disease was less fatal. Among the attendants, twenty-one per cent. of those attacked died: but forty-one per cent. of the others

⁹ The facts were taken from one of the Army Medical Reports—Reports which deal in facts rather than theories—but the date, or the year, was accidentally torn off the extracts.

were carried off—exactly double. And yet, both classes were exposed to the same local or general causes ; while the attendants were exposed, in addition, to the emanations from the sick. So far, then, from these presumed sources of disease being injurious ; we might ask, and with apparently more reason, whether they be not actually beneficial and preservative ? We may leave the contagionists to answer the question : and to show cause against the hypothesis, if they can. There are, it is true, certain facts which tend to show, that ships are sometimes infective, or pestiferous, although the crews are not ; this is an entirely different question, and will be discussed hereafter.

If yellow fever be not spread by means of contagion, it must be ascribed to the operation of some general, but, at present, unknown cause. Take, as an example, an incident mentioned by Humboldt : Two rich inhabitants of the city of Mexico, where yellow fever is unknown, arrived at Vera Cruz where it is endemic, in order to take the packet next day, for Europe. Fearful of catching the disease, although it was not then prevailing in the town, they resolved to pass the night in their carriage, instead of going to an hotel. This availed them nothing. They were both attacked, in the morning, with black vomit, and died before evening. The cause, in this instance, could not have been in the bodies of men : it must have been in the atmosphere.

We may also infer, without much risk of error, that it springs up spontaneously in those countries in which it is indigenous, uninfluenced by individual causes, or by man : more especially as it prevails in uninhabited, the same as in populous districts. That it did so formerly,

in America, would appear certain. Noah Webster states, that "the same pestilential disease (yellow fever) which has lately afflicted our cities, appeared among the aborigines of this country (America) *before* it was settled by the English; before the West Indies were settled by the English or French; and before a single vessel from the Islands had ever reached our shores."¹ As the disease, therefore, must have sprung up spontaneously in these instances, there can be no reason why it should not do the same in all others. With these remarks, we will pass on to another part of the subject, or, rather, to the consideration of another disease—that of small-pox.

Of all diseases this is the one that is considered to be the most contagious—the model contagious disease, in fact. To doubt it would be a medical heresy; one that will place an individual, not in the Inquisition, for that, fortunately, has been abolished; but, under the scalpels of his professional brethren. Still, in spite of this punishment, severe as it may seem, I must avow myself to be a heretic, in this instance as well as in others, having no faith in the doctrine of the origin and propagation of small-pox by contagion, under ordinary circumstances. That it can be reproduced by inoculation is certain, but that is another question: all I maintain is, that small-pox arises spontaneously from some unknown cause, and that it is not propagated, from individual to individual, by contagion. A variety of reasons induce me to draw this conclusion.

We may remark, in the first place, that small-pox is an epidemic, appearing only at particular periods: it must therefore, like all other epidemics, have had an

¹ On Epidemic Diseases, vol. ii., p. 183.

origin. No mention has been made of this disease before the commencement of the sixth century. Alexander Trallianus, who published his work at this period, and who described all the diseases then prevalent, makes no mention of it. Rhazes was the first who wrote on the disease in Europe, at the commencement of the tenth century; but he states that it had been noticed by Arabian writers previously. Montfalcon observes that Ahron, a physician of Alexandria, first mentioned the symptoms, the different varieties, and the treatment of small-pox, at the commencement of the seventh century. According to an Arabian MS. in the library at Leyden, referred to by Dr. Mead, the malady appeared, for the first time, in Arabia, in 572, the year of the birth of Mahomed. But Marius, Bishop of Avenches, states that it prevailed in France, in 570 (two years previously), and again in 580—the wife of Gontran, King of Burgundy, having died of the distemper in the latter year.² We have no accurate account of its appearance in England before the ninth century, while it appears to have been unknown in Norway, Lapland, &c., as late as the end of the thirteenth century. According to the census of Ireland, it appeared there in 760.

Now this was precisely the period when the plague, or black death, made its appearance in Europe; while both diseases have continued to prevail from that time to the present, and apparently, under precisely the same circumstances. We may, therefore, infer that the cause of the production of the two diseases is, in all probability, the same. Sydenham has made the remark, that when there are a number of cases of small-pox in the autumn,

² *Historia Francorum Scriptorum*, &c., vol. ii.

the fever, then prevalent, partakes of the inflammatory character of the former. The only difference is, that, in the one case, there is an eruption ; in the other not. The abundant perspirations and the increase of the salivary secretion, which were met with in each disease, prove, he thought, the identity of the two.³ Be this as it may, there must have been a cause for the production of small-pox in the first instance ; or, in the first case, irrespective of contagion. Granting that small-pox was first generated in the body of some unhappy mortal, and then propagated from individual to individual ; how are we to account for its cessation and its reproduction at subsequent visitations ? By the same means ? That would seem to be hardly probable, hardly possible ; when we find, that this epidemic has continued to return, at certain intervals, during the last twelve centuries, in thousands of instances, in every variety of climate, and among all the different races of mankind. It would be contrary to all analogy to suppose, that the same poison could be accidentally produced under so great a variety of circumstances, and among all these various races—some living on a purely vegetable diet ; others, on an almost exclusively animal one. Nor can we imagine, that the germs of the disease have remained latent, either in animate or inanimate objects, during the intervals of the outbreaks—the period being too long, in most cases. In Mexico, according to Humboldt, the disease only returns every seventeen or eighteen years. Sometimes, the interval has been much longer. In Iceland, there was no visitation of small-pox from 1672 to 1707, or from 1707 to 1785—seventy-eight years. Besides, this disease prevails, not only in warm

³ Chap. ii., on Epidemic Diseases.

climates, but, as we have just seen, in cold ones ; not only in Iceland, but in the Arctic regions, among the Esquimaux—situations in which the germs, or virus, of contagion, could not exist, in a free state, out of the body, for a single day, or hour. And yet the disease, in these latitudes, is not less virulent than in warmer climates. In Iceland, in 1707, 18,000 are said to have perished, out of a population of only 50,000. If, therefore, the disease were not imported—and that would be all but impossible in such localities, not only shut out, to a great extent, from intercourse with other regions, but separated from them by a distance too great to allow of the transport of a patient from one situation to the other—we must infer, that it arose spontaneously. This granted, we may also conclude, that it arises spontaneously in all other instances ; not only for the reasons before mentioned, but, also, because it would be unphilosophical to suppose, that there are two different causes in operation, productive of precisely the same result. As such, it only remains to consider, whether small-pox, after it has arisen, is capable of being propagated from individual to individual, or from the sick to the healthy.

If there be one disease, more than another, which would render an attempt to prove its non-contagiousness difficult or useless, it is precisely small-pox. Unlike the majority of other diseases, there is here actual proof, ocular demonstration, of the presence of a contagious matter, by the formation of pustules on the surface of the body. And yet, it is, perhaps, this very circumstance that renders the disease non-infectious. Assuming, and this would appear to be the only inference that can be drawn on the subject, that small-pox is

produced by the operation of a specific poison, and its introduction into the system ; we may also infer, that the pustules, on the surface of the body, are also due to the presence of the poison in the minute capillaries of the skin. There would seem to be no other way in which to account for their production ; unless we suppose, that the pustular fluid is formed in the blood, and is thence expelled by the secerning vessels on the surface of the body. But, were this the case, the blood would not only become so changed, but also so infectious, that death could only be the result of an attack of small-pox ; and this, too, before the appearance of any pustular eruption. Hence, if small-pox be the effect of a specific poison, introduced into the system from without, we may also infer, that the morbid agent, or the greater part of it at least, is contained in the capillaries of the skin, or in the pustules, during attacks of this disease.⁴

If these conclusions be sound, what is there to fear ? The agent productive of the disease, or the greater part, is locked up—hermetically sealed—in the capillaries of the skin, from which it will be unable to escape unless the pustule bursts, which only occurs in the more severe cases. That the matter, which then escapes from the pustules, is infectious, it would be superfluous to assert : but, then, it is necessary that this matter should be applied to an abraded surface, or be introduced into the blood, by means of an incision, in order to produce any specific effect. Added to this, it is only at a particular period, the first stage, or so, that the pustular fluid would seem to possess infective properties—precisely the period before the pustules break. Afterwards, the matter evi-

⁴ See *Antidotal Treatment of Disease* : chap. Physiology of Disease and Small-pox, where this part of the subject is discussed more fully.

dently undergoes some change, as it is actually absorbed into the blood, when the pustule does not break—it being by the absorption of the fluid part that the pustule dries up. As, also, it is precisely in cases of recovery, that this effect is observed, it shows that the matter contained in the pustules, in the last stage of the disease, cannot be possessed of any injurious or infective properties.

Whether these conclusions be sound, or unsound—and they are entirely hypothetical—we know from practical experience, that the matter discharged from the pustules, in small-pox, is quite innocuous under ordinary circumstances. It is constantly applied to the skin and the hands of nurses, attendants and doctors, without producing any injurious effect. As such, the only way in which small-pox could be propagated from individual to individual, excepting in particular cases, would be either by means of the expired air of the sick, or else by means of the exhalations given off from the surface of their bodies. But is it propagated in either of these ways? This is a question that it is necessary to answer.

We have been told by the late Dr. Gregory, Physician to the Small-pox Hospital, that of the numerous cases received into that establishment, “not one in twenty is capable of being referred to any known source of infection, the disease being ascribed, by the patient, to cold, fatigue, change of air, or some other innocent circumstance.”⁵

If the expired air of a small-pox patient or the exhalations from the surface of his body were infectious, we should expect to find, that the attendants in hospitals, and in private houses, as also the doctors, would be more predisposed than others to attacks of the disease. But

⁵ Cyclopædia of Practical Medicine. Art. Small-pox.

the contrary is the case.⁶ Then, again, if infectious matter were given out from the lungs of such patients, the mortality in hospitals ought to be very much greater than in private practice. In fact, there would be little chance for a person, placed in such an atmosphere, filled with the exhalations from the lungs of twenty or thirty other patients. The result would be as certain, and as fatal, as that of inhaling the pestiferous air, given out from the marshes below the La Ventille Hill at Trinidad. But does such a result occur? Certainly not, for the mortality in hospitals is not greater than in private practice, if we make allowance for the difference in the condition of these patients, when compared with those in their own homes. The former are generally severe cases, while they are, as frequently, sent in after the disease has existed for some time. Taking these circumstances into consideration, it will be found, that the rate of mortality, with the patients thus crowded together, is not greater than with isolated cases. We may therefore conclude, that no pestilential matter is given out by the breath, or from the bodies of small-pox patients; and, consequently, that the disease is not propagated in this way. Another circumstance which tends to prove, that contagion has nothing to do with the propagation of small-pox, is the existence of sporadic cases. If we look through the old bills of mortality, it will be apparent, that there is no year, in which such cases do not occur. How is it, then, that the

⁶ It may be said, and in fact, has been said, that the exemption of these persons is to be referred to vaccination. If this be the case, how are we to account for the attacks of small-pox with other classes that have been vaccinated? In the late visitation, *two-thirds* of the patients admitted into the small-pox hospitals had been previously vaccinated.

disease is not propagated at those periods the same as at others, or when it prevails epidemically? It cannot be for the want of subjects, or the want of susceptibility; inasmuch as the very persons, who then escaped, are precisely those who are subsequently attacked. The only way to account for the phenomenon is, by supposing that there is some general cause productive of small-pox; and that this cause is in operation to a great extent at one period, and to a small extent only at others. As Dr. Prus, the able Secretary to the Plague Commission, has remarked: "*When certain conditions of the atmosphere are absent, a first case of small-pox is seldom followed by a second. We observe this daily, in the hospitals of Paris, although the small-pox patient be placed in the midst of other and ordinary patients.*" With these facts before us, we may reasonably infer, that the diffusion of small-pox is to be ascribed to a general and external, not to an individual and internal, cause.

Although there are several other epidemic diseases, which are set down as contagious—as scarlet fever, measles, &c.—it is not necessary to discuss the validity of this conclusion now. If small-pox be not contagious, we may rest assured that these affections are not. Besides, the arguments that apply to one disease, more especially to small-pox, will be equally applicable to the others.

There is, however, another epidemic which is of much interest in the present day, more so even than that of the plague; for while the one is confined to certain regions of Africa and Asia, the other prevails almost universally,—in Asia, Africa, Europe, and America. This is the epidemic cholera, which is also considered to be a contagious disease.

When observers in Europe learnt, that a new and inscrutable malady had suddenly sprung up in the delta of the Ganges, in 1817, and when they found that it was progressing regularly and slowly across the continent of India, it was natural for them to conclude, that this *nova pestis* was propagated, as the old one was believed to be, by contagion. This opinion was strengthened by the fact, that it spread, at the same time, in the opposite direction, along the eastern coast of the Bay of Bengal to Arracan and the peninsula of Malacca. It reached Siam in 1820, and the Burman empire in 1823. Sumatra, Java, Borneo, the Philippine Islands, and the Moluccas were simultaneously attacked, and, soon after, Macao and Canton. From this point, the cholera travelled to Pekin, which it reached in 1822, and, soon after, Kiachta, the great emporium of the Russo-Chinese commerce; continuing its route in this direction, it traversed the great wall of China, in 1827, and attacked Cocu-Coton, a town in the great desert of Cobi. When, also, the epidemic, by a sudden bound, traversing the intermediate sea, appeared at the mouth of the Persian Gulf, and spread along the shores of this gulf until it reached Asia Minor and Syria, strong confirmation appeared to be given to the above conclusion. But, although the epidemic extended to the ports of Asia Minor, and thus as it were confronted Europe, those who were watching its progress were surprised to find that, instead of spreading farther in that direction, the disease suddenly disappeared.

It was precisely here, in the centre of commercial traffic and human intercourse between Asia, Africa, and Europe, that a contagious disease might have been

expected to spread; and a somewhat startling negation was thus given to the doctrine of contagion. When, also, it was afterwards discovered, that the cholera had progressed regularly and slowly from the Persian Gulf to the Caspian Sea, by a route along which there is scarcely any commercial traffic, and among a scattered population between whom there is little intercourse, and when, instead of spreading from the shores of the Mediterranean, as the plague invariably did, the cholera suddenly sprang up in the southern provinces of Russia, and spread with unexampled rapidity to all the countries of Europe by a circuitous and extraordinary route, until it had reached the northern shores of the Mediterranean, even the advocates of the doctrine of contagion were obliged to confess that the phenomenon was inexplicable.

“Of all the countries in the old world,” remarks M. Moreau de Jonnes, “Russia seemed to be the least exposed to the irruptions of epidemic cholera. Of her European provinces, that which is nearest to the delta of the Ganges, where the malady first appeared, is 1200 leagues in a direct line, and more than 2000 by the track of ordinary communication. Her situation in the highest latitude necessarily limits the period of the hot season, and produces an extreme degree of cold during winter, which has the double effect of diminishing the duration and the violence of foreign contagions—a necessary condition for the existence of which is an elevated temperature. Her commercial relations do not extend to tropical regions, nor even to eastern countries, whence all the pestilential maladies come. Lastly, her population, scattered over an immense surface, and less in density than the inhabitants of Belgium or Lombardy, in the proportion of one to ten,

is, in comparison with all other parts of the continent, that which offers the fewest facilities for the propagation of contagious maladies; and yet, such is the course of events, and the uncertainty of human affairs (or rather of scientific deductions), that the Russian provinces are the first which experience the fierce attacks of Asiatic cholera, and it was by them, that this remarkable scourge first invaded the countries of Europe.”⁷

Independently of these anomalies, there is another question that it would puzzle the contagionists to answer. This is, why the epidemic cholera should have pursued a route diametrically opposed to that of the plague, and have travelled from south to north, on reaching Asia Minor, instead of from east to west? The phenomenon is still more inexplicable, as the epidemic reached Egypt, in 1831, and thus confronted Europe a second time, without spreading farther or reaching the northern shores of the Mediterranean. This shows that epidemic diseases are governed by laws of their own—laws that are not only irrespective of, but entirely opposed to those of contagion. This was rendered evident from the facts observed in Russia, previously to the general outbreak in 1830. Astrakan, a large and populous city on the northern shore of the Caspian Sea, was invaded as early as 1823; but the epidemic subsided without spreading beyond, and did not reappear until seven years after. In August, 1829, also, the epidemic suddenly appeared at Orenburg, the capital of the province of that name, and 400 miles north of the Caspian Sea. From Orenburg it extended about 200 miles northward, and about the

⁷ *Rapport au Conseil supérieur de Santé, sur le Choléra Morbus.*
Par. M. Moreau de Jonnes—A strong contagionist.

same distance in a north-west direction ; but on the 23rd of February the disease was everywhere extinct. It may also be remembered, that the epidemic reached Kiachta, the great emporium of Russo-Chinese commerce, in 1827 ; but, although its ravages here were great, Russia was not invaded from this source. Not only did the disease then subside, without spreading farther, but we have never heard of its extending from this central point since.

Why was it that the inhabitants of Russia were unable to propagate the cholera in 1823, 1827, and 1829, beyond certain well-defined boundaries, although no measures were taken to prevent its extension ; and yet were able to sow the seeds of the disease broadcast in 1830 ? When the contagionists can explain this, sceptics like myself may perhaps have some faith in the doctrine. In the short space of sixteen months, the epidemic extended from the shores of the Caspian Sea to Great Britain, infecting all the intermediate countries—Austria, Poland, Prussia, Germany, Denmark, and Holland—leaving few spots over this vast extent of territory unvisited. During this course, it passed directly across a barrier between Poland and Prussia, formed by 100,000 men, and a triple line of bayonets, so rigidly kept, that a cat, it was said, could not have passed across with impunity, thus laughing to very scorn the puny efforts of man to arrest its progress.

Notwithstanding these facts, the contagious nature of the disease was implicitly believed in, and acted on. The two English commissioners—Drs. Barry and Russell—who were sent to Russia to investigate the nature of the disease, declared in favour of its contagious nature. In consequence, the first cholera board established in Eng-

land, of which Drs. Barry and Russell were members, recommended the Government to establish quarantine, and all the other measures emanating from a belief in this doctrine. This was done, although the profession in India, including the three medical boards, had pronounced *against* the contagious nature of cholera, several years before. When appealed to by the authorities on the termination of the third visitation in 1821, there were only three among the 200 or 300 medical men then in India, who believed in the contagiousness of the disease, and two of these modified their opinion afterwards. These measures having proved abortive here as elsewhere, and their vexatious nature having become irksome, and intolerable to all classes, an outcry was raised, and quarantine, with all other repressive measures, were abandoned. What was the result? Instead of a fourth or a third of a million of the inhabitants of this densely populated city being cut off, only 3000 died; not more than some cities have lost in the course of a few days, with only a twentieth part of the population.

On the next visitation of cholera, in 1848, a very different doctrine—a non-contagious one—was proposed, and an attempt made to prove that the cause of this new disease existed, not in the bodies of men, but on the surface of the earth: in other words, that the cause was to be ascribed to the decomposition of organic matter. This theory, which, to say the least, is a very simple one, had been proposed, some years previously, by Dr. Smith, in order to account for the prevalence of fevers and other ordinary or endemic diseases. Having written a work expressly to point out the unsoundness of this theory, or what is called, Sanitary Reform, it is unnecessary to re-

produce the arguments then employed, or to state the conclusions then arrived at. It will be sufficient now to add, that although my opinions were considered not only heretical but were received, by the majority of critics with scepticism, sometimes with derision, the truth of my conclusions has since been abundantly proved by experience, and by the testimony of sanitarians themselves. Not only have the very measures proposed and carried out for the express object of preventing disease, been actually provocative of outbreaks of fever and cholera, but the majority of sanitarians, including the great apostle of sanitary reform himself—Mr. Simon—have adopted another, and a totally different theory. They have become, *horribile dictu*, contagionists. In 1854, this gentleman, in one of those sanitary proclamations *à la Napoléon*, addressed to the good citizens of London—the purport of which might have been comprised in three words: *Veni, vidi, vici*—thus wrote: “For the permanent avoidance of epidemic diseases, cleanliness is the sole remedy.” By this was meant the removal of decomposing matter on the surface. Hence it was added: “The general liability of London to suffer from epidemic visitation will cease, whenever an efficient and inodorous system of drainage is established.” But now, in order to guard London and England from visitations of cholera, the coasts are to be watched; ships are to be placed in quarantine, patients on shore are to be isolated, while, among the dangers which have to be guarded against, as favouring the spread of cholera infection, are, “outflow, leakage, and filtration from sewers, house-drains, &c.”⁸

⁸ Mr. Simon's Report, 1874. “Memorandum of Precautions to be taken against the *Infection* of Cholera.”

So that, what was before pointed out as a panacea for all the ills to which flesh is heir, is now regarded as a source of danger—the cause of death rather than that of health.

This change of front, to use a military term, had been brought about in the following way. The authors of sanitary reform concluded, that not only the emanations from decomposing matter on the surface, but the presence of the same matter in drinking-water, was one of the principal causes of cholera and other diseases. But the late Dr. Snow inferred, that mere impurity of the water would not cause cholera, unless it were of a specific kind. His conclusion was, that the *materies morbi* of cholera is something which passes from the mucous membrane of the alimentary canal of one person to that of another : this it can only do by being swallowed. This was effected, according to Dr. Snow, by means of the morbid matter contained in the evacuations, finding its way from the sewers and drains into the drinking-water. This theory was at once adopted by Mr. Simon, although it would seem to be highly improbable, that all the water used by a large population, deriving its supply from a variety of sources, could be contaminated in this way, even supposing that this cause were sufficient to account for the spread of cholera. Then again, the cholera has prevailed in many countries where there are neither drains nor sewers, and where the contamination of the water from such a source was utterly impossible.

Dr. Snow also inferred, that the germs of the disease passed into the system in other ways, with the food, and from the soiled hands of nurses, &c.—a very nasty idea,

if not a nasty theory. It, however, was adopted by Mr. Simon, who added, "The infective influence of choleraic discharges attaches to whatever bedding, clothing, towels, and the like things have been imbued with them." Were this the case, nurses and other attendants on the sick could hardly escape an attack of cholera, whereas they have invariably been more exempt from the disease than any other class.

It would thus appear, that we are now at the end of the nineteenth century verifying the saying of Bacon, that medical men move like the horse in a mill, in a circle; returning always to the point from which they started. Unlike the horse, they do not appear to effect much good during these revolutions: there being, according to his lordship, *much iteration and small progress!* Not only have we returned to the doctrine of the middle ages, but we shall soon, no doubt, follow their practice. The thin edge of the wedge has, in fact, been already inserted; for the authorities have now power to remove the healthy and isolate the sick, during outbreaks of cholera.⁹ The corpse also is to be buried "*with the earliest possible despatch,*" which means, we may presume, as soon as the breath is out of the body, in accordance with the practice adopted by the inhabitants and the negroes in Jamaica. The same rules and directions apply to ships, which are to be placed under the orders of the vestry, local board, or nuisance authority—a very proper designation! With quarantine, isolation of the sick, speedy burial, and its concomitant, the dead cart, all that remains will be to paint a *red cross* on the infected houses, or places, as in days of yore, with the inscription under:

⁹ See Mr. Simon's Ninth Report, *loc. cit.*

“Lord, have mercy upon us.” This means, Lord, have mercy on the poor creature, thus isolated, and cut off from all human aid, and all human consolation! Perish the thought!

Such being the opinions of Mr. Simon, we need not be surprised to learn, that the outbreak of cholera at Southampton, in 1865, was referred by him to importation. “In the summer of 1865,” remarks Mr. Simon, “cholera approached us from the Mediterranean: Egypt had been badly *infected* by Mohammedan pilgrims, returning from Mecca: the infection had spread along the lines of steam-boat communication, which diverge from Alexandria, as a centre, to all the most considerable ports of the Levant and of southern Europe; and, presently, as was expected, a first wave of the infection touched our shores. For the first time in our experience, the attack was on our south coast. Into Southampton there came, on July 10th, and at intervals afterwards, very *suspicious* arrivals from Alexandria, Malta, and Gibraltar.”¹ This is in accordance with an axiom previously enunciated—viz., that, “contagious current on the continent of Europe must be deemed virtually current in England.”² This, we are further informed by Mr. Radcliffe, is to be accepted as an *axiom* in State medicine.³ Not being aware that there was a contagious current on the continent of Europe; and not having been initiated into the mysteries of State sanitation, it is necessary to pause a short time, in order to inquire what the facts are on which these

¹ Twelfth Report of the Medical Officer of the Privy Council, 1870, p. 28.

² Eighth Report, p. 43.

³ Ninth ditto, p. 228.

official *dicta* repose. As, according to Mr. Simon, the infection came originally from Mecca, it will be better to go at once to the *fons et origo mali*, in order to verify the validity of the preceding conclusions.⁴ The history of this outbreak has been given by Mr. Radcliffe, in the Report just alluded to; and,* also, by Messrs. Adams and Welsh, Army Surgeons, then stationed at Malta.⁵ From these reports, we glean the following facts.

Pilgrims to the number of 200,000 had assembled at Mecca, early in the spring; soon after which cholera broke out among them, carrying off, it is asserted, from 10,000 to 15,000. That the cholera was not imported into Mecca, but that it arose there spontaneously, we may infer from the following facts. The ceremonies do not last many days: and the principal one—the Kurban Bairam, or Feast of Sacrifice—took place on the 4th of May. But the cholera had appeared as early as March at Makhalla, a port on the Red Sea; and it was here that some pilgrims from Singapore were attacked on their arrival. These pilgrims, amounting to 1066, nearly all Javanese, had arrived in two sailing-vessels—the *Persia* and the *North Wind*. “The disease,” as we are informed by Dr. Sawas Effendi, “broke out on board, after touching at Makhalla, and after the crew and passengers had partaken

⁴ The origin of alleged contagion has always been a subject of dispute. The Americans refer the yellow fever to the West Indies, and the West Indians to Africa. The Russians accuse the Asiatics of introducing the cholera and the cattle plague, whereby the other inhabitants of Europe conclude that the Russians have conveyed both these diseases to them.

⁵ Army Medical Report for 1865.

largely of fish of a bad quality, and of brackish water.”⁶ According to Mr. Calvert, H.M. Consul at Jedda, the epidemic was prevailing, at the same time, at Hodeida, another port in the Red Sea. We have thus proof that the cause productive of the epidemic cholera was in operation in this part of the world before the arrival of any pilgrims. As such, if that cause—no matter what it may be—was in operation in other and adjacent towns, the probability is, that Mecca was brought under the influence of the same malign agency, although it might not manifest its effects until a later period. If so, there can be no difficulty in accounting for the outbreak. The disease arose here spontaneously, the same as in other places; and the pilgrims were attacked, in common with the inhabitants, possibly before, being more predisposed than the latter, for a variety of reasons.

The pilgrims fled when the disease broke out, and, as a matter of course, many of them took the seeds of it with them: they fell sick on the road, or in the nearest town. Mr. Consul Calvert, in a letter to H.B.M.’s Consul-General in Egypt, states, that the mortality was great among the pilgrims on the road from Mecca to Jedda. The survivors arrived in this town on the 10th of May, but the epidemic was then prevailing there, many of the European sailors and Lascars having died previously, as, also, some of the Turkish soldiers in garrison. The disease consequently was not carried into Jedda by the pilgrims. The same result no doubt, occurred on other roads taken by them, but no account has been transmitted to us of these occurrences. The majority of the pilgrims go to Egypt, and thence to their several destinations;

⁶ Sur la Marche et la Mode de Propagation du Cholera en 1865.

and the first ship, with 1500 passengers on board, arrived at Suez on the 19th of May. The Egyptian Government had taken the precaution to send medical men to Suez, to examine the pilgrims on their arrival; and they reported, that no indications of cholera existed among them. Some deaths had occurred on board during the voyage, but the cause was not known. We only learn that the captain and his wife were both attacked on the 21st, two days after the arrival of the ship at Suez.⁷ After this, we hear nothing more of the pilgrims, not a single case having been reported among the thousands that arrived in Egypt: had there been any, we should most assuredly have been informed of the fact. On the 2nd of June there was a case of cholera, with a *resident* in Alexandria; on the 5th, two others; and, on the 11th, the existence of the disease in Alexandria was officially announced. By the end of the month, nearly all the towns in the delta of the Nile had been attacked—Cairo, Rosetta, Damietta, &c.—spreading, at the same time, beyond the Egyptian boundaries, to Malta, Smyrna, and Constantinople. As the epidemic thus broke out in Egypt, so soon after the arrival of the pilgrims, the contagionists of course affirm, that the germs of the disease were brought there by them. Had this been the only visitation experienced in that country, it might be somewhat difficult to disprove the assertion. But Egypt has been invaded by cholera on numerous occasions since 1831, the first outbreak: while, in the majority of instances, there were no pilgrims to carry the infection—the epidemic having appeared at a different

⁷ It has been stated, on the authority of a private practitioner in Egypt, that one of the pilgrims was attacked with cholera on the journey from Suez to Cairo. This is not improbable.

time of the year to that of the pilgrimage to Mecca. This will be rendered apparent, by an inspection of the Table now added.

TABLE.—*Date of the assembling of the Pilgrims at Mecca, and that of the Outbreak of Cholera in Egypt, in the following years.*

Feast of Sacrifice.	Outbreak in Egypt.
18th May, 1831 . . .	July, 1831.
10th November, 1847 .	24th June, 1848.
20th October, 1849 . .	25th July, 1850.
24th August, 1855 . .	4th June, 1855.
4th May, 1865 . . .	2nd June, 1865.

We thus find, that in only one of the above instances did the outbreak of cholera, in Egypt, occur after the dispersion of the pilgrims at Mecca. In two of the other instances, the assembling of the pilgrims at Mecca occurred seven and nine months before the epidemic made its appearance in Egypt: while, in the third instance, the cholera actually appeared in Egypt two months before the arrival of the pilgrims at Mecca. In these instances, therefore, these unjustly accused and maligned persons could have had nothing to do with the propagation of the cholera in Egypt, whatever may have been the case in the first example, that in 1831. It is also to be remarked that the epidemic sprang up about the same time of the year, in each instance; the same when there were pilgrims as when there were not. This shows, that this modern scourge observes its own laws, irrespective of human agency and accidental circumstances. If, therefore, the disease could

spring up spontaneously in Egypt at one time, it could do so at another: and this is the only logical deduction to draw on the subject. The simultaneous arrival of the pilgrims, and the outbreak of the disease, must be regarded, not as a cause, but merely as a coincidence. In fact, the epidemic only pursued, in 1865, the route it has invariably followed from the commencement, in the first outbreak as well as in the last; that is to say, it progressed from south to north. We never hear of its pursuing the opposite course in Egypt, in Syria or in Persia. And why is this? Simply because it is a law of the disease: one that cannot be explained by a reference to the doctrine of contagion. If pilgrims and others can transport the germs of the disease from south to north, they, or others, would be able to carry them in the opposite direction. As human traffic and commercial intercourse are not, like the magnetic needle, directed always to one point, we may conclude, that the epidemic cholera was not transported from Mecca to Egypt, by the pilgrims in 1865.

We will now trace the course of the disease westward; the epidemic having spread, this year, contrary to its previous habit, to the northern shores of the Mediterranean, to Italy, France, and Spain. If this were by infection, nothing could have been more erratic than its course. Malta was attacked on the 20th of June, quarantine having been established on the 14th for all ships arriving from Egypt. Previously to this, however, or, between the 1st and the 14th June, thirteen steamboats had arrived, with passengers on board; the majority pilgrims bound for other places,—while a certain number landed on the island. It has been concluded that the

disease was imported by these passengers, the maxim of the contagionists being *post hoc, ergo propter hoc*; although none of them were affected with cholera, either at the time or subsequently. There are, nevertheless, a few facts, that militate against this conclusion. The island of Rhodes, situated between Alexandria, Smyrna, and Constantinople, in all of which places the epidemic prevailed, entirely escaped,—not a case having occurred among the inhabitants, numbering 33,000. And yet, 222 sailing-vessels, and sixty-six steamboats, with 2618 passengers on board, and with crews amounting to 2501 persons, arrived there in the course of two months. With the exception of one case in the Lazaret, with a person who had landed the day before, having arrived in a ship from Alexandria, no attacks are mentioned among either the passengers or the crews of these vessels. The island of Mitelin also escaped, although seventy steamboats anchored there, and disembarked 775 passengers. She also received, in her port, 235 ships, with crews amounting to 1420 men: but these vessels were all placed in quarantine. These are not the only anomalies that occurred. At Trieste there were eighty cases and sixty deaths: and although the epidemic prevailed slightly in some of the surrounding villages, the disease did not spread beyond. At Ancona a quarantine of seven days had been established. Nevertheless, there were 3763 attacks of cholera, and 2108 deaths, in this town, while the epidemic spread into the province. So that, the disease entered where a strict quarantine was established, and then spread into the interior; but did not spread, where no precautions were adopted to prevent its diffusion—at least at Trieste.

Another curious anomaly is, that the epidemic commenced at Marseilles at the same time as in Egypt—the beginning of June. The outbreak at Marseilles has been ascribed to the arrival of a ship there (the *Stella*) on the 11th June, from Alexandria, with ninety-seven passengers, of whom sixty-five were pilgrims bound for Algiers. Now this ship left Alexandria on the 1st June, some days before the cholera broke out there. As such, if the disease was carried to Marseilles by the pilgrims, they must have imbibed the seeds of it at Mecca, four or five weeks previously. Added to this, as none of these men were sick on their arrival, we are bound to infer, that those seeds which, when scattered over the good town of Marseilles, produced such fatal results, had remained dormant and innocuous in the bodies of the pilgrims all this time. Did these men, we may ask, in all simplicity, possess a charm, like the snake charmers, against poison, and the poison of cholera in particular? If so, their secret would be worth knowing, aye, and worth purchasing, too, at any cost. Besides, it has been before stated, that the poison of plague does not lie dormant in the system more than eight days—and the incubation of cholera is probably less. The infection, therefore, could not have been conveyed to Marseilles by the pilgrims. This will be rendered certain by the following particulars. According to Dr. Grimaud de Caux, there were several deaths from attacks of cholera on the 9th of June.⁸ Dr. Sélim-Ernest Maurin says, on the 7th,⁹ four days before the arrival of the *Stella*: “These cases exhibited all the characteristic symptoms of Asiatic cholera—blueness of the skin, suppression of urine, raucous voice (*voix*

⁸ Gazette des Hôpitaux, 17th Oct., 1865.

⁹ *Ib.*, 30th Sept.

cholericæ), coldness, &c. As such, the disease, instead of having been imported, must have arisen spontaneously in Marseilles. This inference is confirmed by other facts. The cholera broke out at Toulon, a purely naval arsenal; which has little communication with other ports, and to which neither commercial nor passenger ships resort. And yet the ravages of the disease were as great here, if not greater, considering the difference in population, than at Marseilles. In the latter town, the deaths were 1847; in Toulon, 1282. At Arles and Aix, also, the epidemic committed greater havoc, than at the port of its presumed disembarkation. It spread through the south of France, and reached Paris on the 13th October. This appears to have been its boundary northwards, with one exception. The disease broke out at Altenburg, a town about twenty-four miles from Leipzig, but did not extend beyond. Perhaps, the contagionists will be kind enough to inform us, why so infectious a disease was confined to this single spot, the inhabitants not having been placed in quarantine, and no wall having been built around the town?

We will now turn to another part of the world. In Spain, the first place attacked was Valencia, which the disease reached on the 3rd July—seven days before it broke out at Gibraltar. The selection of this place is another anomaly of the epidemic regarded by the doctrine of contagion; for this town could have had no direct communication with Alexandria, or other infected port. What is called the Port of Grao, situated a short distance from Valencia, is merely a beach, at which only small coasting-vessels think of touching. All vessels and ships bound to this part of Spain go to Alicante, where there

is a good harbour. And yet here, where you might have expected the first cases to have occurred, the epidemic did not make its appearance until September—two months afterwards. The disease spread into the interior of Spain, and prevailed rather extensively; 486 towns, villages, and hamlets having been attacked, before the subsidence of the disease on the 1st of November. Madrid was attacked on the 9th of October; the outbreak being so sudden that it can only be compared to some violent explosion. Within the *first three days*, there were 3000 cases, of which number 1000 proved fatal. The termination of the epidemic was equally sudden, having ceased entirely on the 15th—six days after its appearance. To talk of infection, in a case like this, would be sheer nonsense.

With the mention of another anomaly, we may close this part of the subject. As is well known, a large number of the pilgrims, that pass through Egypt, go to Algiers. As such, we should have thought, that the first cases of cholera which occurred on the western shores of the Mediterranean would have been in Algiers. But the epidemic did not make its appearance there until September—three months after the arrival of the pilgrims. Really this disease must be a very ill-mannered one, thus to set at defiance all the laws of contagion, and the rules expressly laid down for its guidance by the contagionists.

Having thus endeavoured to prove, that the epidemic cholera was not introduced into Egypt, and other countries, by the pilgrims from Mecca, we may now return to the point from which we started, in order to ascertain if the disease was imported into Southampton, in 1865,

as stated by Mr. Simon. Fortunately, a special Report on this outbreak has been drawn up by Dr. Parkes, and no better person could have been employed to carry out such an investigation. From this Report, we glean the following particulars; merely observing, that the disease was supposed to have been introduced by one of the steamers belonging to the Peninsular and Oriental Company, on board of which there had been some cases of cholera. According to the returns furnished by the Peninsular and Oriental Company to Dr. Wiblin, the Medical Superintendent at this port, it appears that, of twenty-three steamers which sailed from Alexandria for Southampton, *viâ* Malta and Gibraltar, at a time when cholera was committing most serious ravages amongst the population of those places, there had been only seven deaths from cholera; and these had occurred on board the *Ellora* and the *Nyanza*.¹ Three of these deaths were on the *outward-bound passage*, between Marseilles and Alexandria; the other four on the voyage from Alexandria to Southampton. Two of these were on board the *Ellora*; one death having occurred two days before she left Alexandria, on the 28th June, and the other, two days after. The remaining two deaths occurred on board the *Nyanza*, during the voyage from Alexandria to Southampton. They were both passengers, one of whom died on the 9th July, the day after leaving Alexandria; and the other, on the 17th July, at Gibraltar—five days before the arrival of the steamer at Southampton. It is thus apparent, that there were no cases of cholera on board the regular

¹ Report by Professor Parkes, M.D., on the Outbreak of Cholera in Southampton, 1865. Eighth Report of the Medical Officer of the Privy Council, p. 425.

steamers, at the time of their arrival at Southampton; but Dr. Miller, the surgeon, stated that there were thirty-one cases of diarrhœa on board the *Ellora* during the voyage—the last case having occurred two days before she entered the docks. According to Dr. Wiblin, there was diarrhœa on board the *Nyanza* also, but the number of cases was not known. Still, it does not appear that any one landed suffering from diarrhœa. In answer to a question from Dr. Parkes, Dr. Wiblin replies: “From the most careful inquiries instituted, I am unable to ascertain that any cases, either of cholera or diarrhœa have been landed at the Port of Southampton.”² Had it been otherwise, however, we should then have found it difficult to account for the spread of the disease in Southampton: none of the patients first attacked having had any relation, or communication, with the ships, or shipping. The first case was that of a female, and she was not attacked until the 12th of August, twenty-one days after the arrival of *the last* of the two, infected ships. Then, again, there was no fresh attack until the 22nd of September—an interval of forty-one days. How, then, did this patient receive the infection? It could not have been from the ship, nor yet from the first person attacked. Were it shown, that these patients had been in communication with each other, it would be impossible to infer, that the one had imbibed the infection from the other, the time being too long for the poison to lie dormant in the system. But we are told distinctly that this patient had not been near any other. Dr. Parkes observes: “As already stated, it cannot be shown that Rose, Wescot, or Hembery (the

² Loc. cit., p. 425.

first three persons attacked after the young woman on the 12th August), had been near any other patients. All inquiries have failed in tracing directly their sources of exposure."

While, on the one hand, personal contact with infected persons could not have been the cause of the outbreak, so, on the other, the contamination of the water could not have been the exciting agent. Dr. Parkes remarks: "The supply at Southampton is from the chalk, and is quite free from deleterious matter. As the service is continuous, there is no local contamination from cisterns:" and, we may add, from any other source, as the dejecta of cholera patients. Besides, as "all the persons attacked drank the town water, which was consumed also by the other 53,000 persons in Southampton," the disease would not have been thus limited in its range had the water become contaminated. Nor could the germs of the disease have been preserved in the soil from the overflowing of cesspools, and the extrication of deleterious miasmata into the surrounding air, for all the houses in Southampton are sewered: while "it so happens," remarks Dr. Parkes, "that the sewers act better, in that part of the town where cholera prevailed, than in some wealthier parts. In most of the old small houses, the old privies outside the houses have been converted into water-closets, which have been well trapped. . . . Every evacuation was at once washed away and carried into the sea." The cases, in fact, were too much scattered to refer the disease to any local cause; at the same time that they broke out almost simultaneously at separate points, far removed from each other. The earliest cases—always excepting the first case in August

—were at Weston Common, a small hamlet about two miles from Southampton : the next, in Southampton, and then at Bitterne, a village two miles from the town and one and a half from Weston. There were also cases (6) at a later period at Itchen, a small fishing-village, independently of those in the suburbs of Southampton—at St. Deny's and Freemantle, situated to the south and west of the borough. This is not how an imported and infectious malady would have spread: the first cases ought to have been, not in the neighbourhood, but in the town itself, and with those in direct communication with the ship. From this it should have spread, like radii from the centre to the suburbs; but the directly opposite course to this was pursued. Then, again, if the disease be so contagious, that a ship, on board of which there had only been two cases of cholera, could infect a town, how are we to explain the exemption of the 52,940 persons out of the 53,000 inhabitants of Southampton? Instead of two patients, whose bodies had been committed to the deep some days before the arrival of the ship, there were here sixty cases, or *foci* of infection, as the term is, scattered among a large population. And yet, with the exception of sixty, all these 50,000 persons escape an attack; without it being possible to refer the exemption to any apparent cause. Referring to the cessation of the disease at Weston Common, Dr. Parkes observes: "These five cases were all that occurred in this locality. Nothing whatever was done to arrest the disease, and there were numbers of persons in the adjoining houses, who must have been susceptible, yet it did not spread. The causes of the cessation lay, therefore, in no preventive action."—(p. 401.) Had the

cholera been "stamped out," according to the last patent method of preventing disease, and had the 52,940 uninfected persons in Southampton been sacrificed on the altar of scientific ignorance and popular credulity, we could have understood the cessation of cholera in this town. At present, however, it remains as much a mystery as the origin of the disease.

That this importation theory is an inconsequential one, will appear to be more particularly the case, if we turn to other facts. In the quarter ending June, 1865, there had been 32 deaths from cholera, and 706 from diarrhœa, registered in London. These are merely the deaths, how many cases of cholera there were we have no means of ascertaining. One of the latter has been recorded, and it will be interesting to give the particulars, as showing that some of these cases, if not all, were really attacks of Asiatic cholera. In May, 1865, a case, described as "Asiatic cholera" was admitted into Guy's Hospital under the care of Dr. Wilks. This case was reported in the *Medical Times*³ and the reporter stated that he had employed this term, because "it was such a case, as would have been so called during the prevailing epidemic." And he added: "Dr. Wilks said, that, *every year*, he saw one or two such cases, but seldom so early in the year as this." Whence, then, did these cases receive the infection? Not from Southampton, nor yet from Alexandria, for they occurred not only before the arrival of the steamer at the former port, but some of them before the outbreak in Egypt, which did not commence until the beginning of June. Nor could they have received the germs of the disease from any

³ June 3, 1865.

other source, as the epidemic did not make its appearance in the south of Europe until July, and in the north, until September. Granted that the infection had arrived by electric telegraph, or otherwise, from the ports of the Red Sea, infected in April and May, we should, even then, be at a loss to account for the cases and deaths that occurred previously to this. Independently of the cases already mentioned, there were 934 deaths from cholera, and 16,432 from diarrhoea, registered in England, and 156 from cholera, and 2364 from diarrhoea, registered in London, in 1864. As this was actually before the epidemic appeared in Egypt, and before it recommenced in Russia or any other country in Europe, there was no possible source to which we could look for the importation of the disease. Besides, if the disease had been introduced by infection, it would have spread by infection, after its arrival. But this was not the case, as the cholera did not become epidemic in England until two years after. Under these circumstances, the only conclusion to be drawn on the subject is, that the epidemic cholera arose spontaneously in England in 1864, and in Southampton in 1865.

If therefore the cholera arose spontaneously in England in 1865, we may also conclude that it sprang up spontaneously in France in the same year. That the cholera was not imported into this country, that year, will be shown very clearly if we refer to the visitation of 1884. In this instance, the question of the importation of the disease from Egypt into Europe is negatived by the fact, that it had subsided in the former country twelve months before it broke out in France and in England, nor could any other source of infection be discovered.

The contagionists, who, like drowning men, are ready to catch at any straw that may be floating on the surface, suggested that the *Sarthe*, which had recently arrived from Tonquin, and on board which two cases of cholera had occurred while at Saigon, might have brought the disease. But these patients were sent on shore, as soon as attacked; the ship was fumigated and no cases occurred afterwards. In addition to this, an interval of seventy days had elapsed—from the 1st of April to the 10th of June—from the occurrence of these cases to the arrival of the ship at Toulon, while no cases had occurred on board subsequently. Nor could the disease have been imported from Egypt, the epidemic having subsided, in that country, more than a year. In addition to the above, Dr. Bonardel, who had been sent to Toulon to investigate the cause of the outbreak, stated at a meeting of the Academy of Medicine, Paris, July 2nd, 1884, that the first case was that of a sailor, on the 14th of June, on board the *Montebello*; and the second, that of another sailor on board the same vessel, on the following day. Neither of these men had had any communication with other vessels, or even with the shore, previously to being attacked. As such, this inference therefore had to be abandoned; and the medical commissioners, sent by the French Government to investigate the cause of the outbreak, concluded, that the cases were sporadic, i.e. local. But the epidemic soon spread to Marseilles, Aix, Montpellier, and nearly all the towns in the south of France, although its progress was slow, and the cases and deaths insignificant and few compared with the population. Having reached Nîmes, it appeared, for the time, to be arrested in its march northwards. In fact, Dr. Proust, the Inspector-

General of the Sanitary Board, in his Report, addressed to the Minister of Commerce, the middle of September, stated, that the epidemic was disappearing from France. But, on the 3rd of October there was a fatal case at Yport, Normandy, speedily followed by several others, although no cause could be assigned for the outbreak; the first persons attacked having had no communication with infected persons or with infected towns. It spread on the 23rd to St. Leonard, a village between Yport and Fecamp, and, unlike the former, a model of neatness and cleanliness. On the 24th of October the epidemic suddenly appeared at Nantes, the first case being of a *foudroyant* character, while from that date to the 30th there were twenty-five cases, nearly every one of which was fatal. Notwithstanding this malignancy of the disease, the number of cases and of deaths was very limited. From the 31st of October to the 20th November, when there was one death only, fifty deaths were recorded and none afterwards. Allowing that all the cases were not reported, still, the mortality was ridiculously small for a contagious disease, when introduced into a populous town like Nantes.

During this period, or in the month of September, there had been some cases and deaths in the suburbs, and six in the neighbourhood of Paris; two cases at Clichy, one death at St. Denis, one at Asnières, two at St. Ouen, and one at Abervilliers. As no cases were reported afterwards, the disease must have subsided as suddenly as it appeared. But on the 5th of November, a young woman, living in the centre of Paris, and who had been entirely removed from any source of infection, was suddenly attacked with all the symptoms of Asiatic cholera, and died after a few hours' illness. From

this time, the disease continued to spread, and prevailed until the beginning of December; but its range was very limited, and its ravages almost infinitesimal. On a rough calculation, less than 2000 persons were carried off by cholera in a population of about 2,000,000.

Thus ended the epidemic of 1884 in France. Reviewing the preceding facts, the circumstance, that will strike even a casual observer, is the limitation of range of the epidemic, both generally and locally, and the small, insignificant mortality. If we compare the spread of the epidemic cholera in 1832, with that in 1884, a very striking difference will be apparent. Cholera appeared at Calais on the 15th of March, 1832, and broke out at Arles on the 17th of September following, having thus in 186 days, traversed 200 leagues, forming the great diameter of France, from north to south. The disease was recognized at Paris on the 24th of March, eight days after its appearance at Calais. On the 17th of April, it had spread by contiguity to the department of the Moselle, and on the 11th of May, to that of Finisterre, taking thirty-five days to reach the eastern, and fifty days to reach the western frontier of France—having traversed, on the one side, seventy leagues, and on the other, 120. Thus, the cholera traversed the kingdom of France, from north to south, at the rate of one league in twenty-four hours; whilst, from east to west, it required but eight-five days to travel a distance of 100 leagues, which gives a rapidity of speed greater by one-half. During this route few places in France escaped, while the mortality was excessive, 18,000 having died in Paris alone. In 1884, however, few places to the north

of Nîmes were attacked, while the mortality has been insignificant. What, then, it may be asked, is the cause of this difference? Dr. Decaisne and other writers have inferred, that the cholera has become milder in type, and hence the lesser mortality, and the limited range of the disease. But this inference is not borne out by experience, or by the history of this particular outbreak.

In India, from 1821 to 1833, the ratio of deaths to cases, among the European troops, was 25 per cent.; but from 1834 to 1846, the proportion was 50 per cent. During the outbreak at Kurachee, in 1846, the mortality in H M.'s 86th Regiment was 58·5 per cent., and in the 88th Regiment, 59·36. In the 93rd Regiment, in 1862, the rate was 70 per cent., being eighty with the officers, and sixty with the men. In 1863, the average rate in Bengal was 75 per cent.—exactly three times more than during the first years of the prevalence of the epidemic in India. So, again, if we turn to the facts presented to our notice during the visitation of 1884, we shall find that the cholera did not assume a milder form. In 1832, the average rate of mortality in Europe, did not exceed 50 per cent.; but at Yport, of twelve cases, eight proved fatal, being a ratio of 66 per cent. In other instances, the ratio was still higher. Of six cases in the commune of Boufpiques, five proved fatal—a ratio of 83 per cent. At St. Remezy, there were eight deaths from the most aggravated form of the disease; and at Paris, one of the sisters in the alms-house in the Avenue de Breteuil, was carried off in three hours. At Nantes, also, as already mentioned, the epidemic put on a very severe and even *foudroyant* form. It is thus evident, that the comparatively small mortality in

France, in 1884, is to be ascribed, not to the mildness of the disease, but to the limitation of its range. This limitation was still more marked in Spain.

In this country, not more perhaps than a dozen towns were attacked, while the number of cases might be counted on the fingers. Notwithstanding, the attacks would appear to have presented as severe a form as in France, judging from the meagre accounts in the public papers, no official reports having been furnished. Thus, of forty cases reported, there were thirty-one deaths, being a ratio of mortality of seventy-seven per cent.—twenty-seven above the average previously mentioned.

In Italy, there were more towns attacked, and more cases in each, generally speaking, while in one—Naples—the outbreak was as severe as in former visitations. In this town there were 12,402 cases and 6629 deaths; and in the province of that name, 14,037 cases and 7576 deaths. Still the range of the epidemic, in the other provinces of Italy, was very limited, and the ravages of the disease altogether insignificant, compared with the first visitation of the cholera. Numerous anomalies, viewed by the doctrine of contagion, were also observed here the same as elsewhere. For instance, there were three cases of Asiatic cholera in Rome, in the month of September, two of the patients being labouring men, and the third a soldier in the first regiment of Bersaglieri. Neither of these men had been in contact with any cholera patient, or other presumed source of infection, nor did they give the disease to their friends and companions, or spread the seeds of it in this populous city. This is the more noteworthy from the fact, that the first case was supposed to be one of malignant ague—a disease that assimilates

much to that of cholera—the true nature of the attack having only been ascertained by a post-mortem examination. No means therefore were taken to isolate the patient. Another anomaly may be mentioned. At Pescara, a small town on the Adriatic shore, south of Ancona, there were twenty cases and ten deaths, in the course of a few days, caused by a sudden outbreak of cholera. But there were no cases afterwards, the epidemic having subsided as suddenly as it had appeared.

From a review of the preceding facts, we may conclude, that the comparative mildness of the visitation in 1884, is to be ascribed solely to the limitation of range of the epidemic. Why this should have been, and why, in the two visitations in which the cholera progressed in France from south to north, it was unable, except in one solitary instance, to extend beyond the latitude of Paris, is a puzzle, that we may leave the contagionists to explain, if they can. The result is the more remarkable, when we remember the rapidity with which the epidemic spread in the opposite direction, or from north to south, on previous occasions. It is evident, that some obstacle exists to the propagation of the epidemic cholera when travelling from south to north, or from the shores of the Mediterranean—an obstacle that does not exist when the disease spreads in the opposite direction, or from the north to the south. What this obstacle is, it has been my object to point out on a previous occasion, and it will be discussed again in a subsequent chapter. It is unnecessary, therefore, to enter farther into the subject in this place.

Another circumstance shows very clearly that the epidemic cholera arose spontaneously in France in

1884. "It has been ascertained," says the *Lancet*, "that cholera made its appearance last year—1883—at Marseilles; a case of cholera, followed by death, having occurred at the hospital, in a room containing numerous other patients. The disease made a good many victims. . . . In order to avoid panic in the town, the mayor solicited and obtained the silence of all persons aware of the facts. The head surgeon, two house surgeons, a warder, and two sisters of mercy, took an oath to divulge nothing; and this oath was strictly kept till the 4th instant, when the circumstance was made known to the medical committee—August 9th, 1884. Not only are these isolated cases observed previously to an outbreak, but they are also observed after its subsidence, and that too almost invariably. Thus it was that, after the extinction of the epidemic in France, in 1884, five cases of cholera occurred at Tubervilliers, of which two were fatal. At St. Denis, in the poor-house, seventeen cases occurred in the same month, January, 1885; and at Asnières, several cases of a choleraic character were noted. In fact, there are few years in which there are not some sporadic cases; but it is only in particular years that the cholera, from causes of which we are at present entirely ignorant spreads, or becomes general as these isolated cases present the same type, or intensity, as the others; there can be no reason why the disease, if it be contagious, should not be propagated as readily at one time as another. Independently of the preceding remarks and conclusions, there is another result that may be mentioned, which is in direct opposition to the idea, that diseases are propagated by human contact. This is, that epidemic and endemic diseases prevail to a greater extent

in rural districts than in towns, sparsely inhabited rather than in populous places. There are no statistics to fall back upon, in order to prove the truth of this conclusion as regards the plague, but they abound with respect to other diseases. It may be mentioned, however, that Mr. Howard, the philanthropist, with his extensive experience, was obliged to seek for some other cause than overcrowding, in order to account for the production of gaol fever or plague.

The exemption of sailors from yellow fever, in the confined atmosphere of between decks of a man-of-war, when in contact with those suffering from the disease, and contracted elsewhere, has been already dwelt on. In addition to the instance then mentioned, another may be given here. During the visitation of yellow fever, at Cadiz, in 1764, H.M.S. *Tweed*, remarks Lind, was anchored in the bay. Although a hundred victims were carried off daily on shore, none of the crew of the *Tweed* died. Several, however, were attacked while on shore, but, being taken on board immediately, they all recovered, without communicating the disease to any of their comrades.

Precisely the same results, even in a more marked degree, may be observed with epidemic cholera. It was, in fact, among the wandering tribes of the desert of Arabia, and among the scattered population of the mountainous region of the Caucasus, that cholera, on its first invasion of these countries, prevailed in its greatest intensity, and committed its greatest ravages. In Arabia, a third of the inhabitants, according to Moreau de Jonnes, perished; while in the Caucasus, 16,000, or two-thirds of the population, were

attacked, and 10,000, or nearly half, died. These are not isolated facts. This law was rendered still more evident during the outbreak of cholera in Jamaica, in 1850. At Kingston, with a population of 40,000, not more than a sixth, or fifteen per cent., of the inhabitants were cut off. But at Falmouth, a small town, the deaths amounted to a third. In Port Maria, a still smaller town, two-thirds of the population, or 600 out of 900, perished. "At first," writes the Rev. I. Simpson, "the epidemic was mild in its type, and yielded readily, in most cases, to the treatment of our medical men. But, on the 1st of December, it burst on the town like a flood, carrying off 400, nearly half the population, in the short space of ten days." In the small towns and villages, the mortality was much greater. At Houghton Court, an estate close to Lucea, forty died in the course of ten days, when the remainder, panic-stricken, fled. Of these, many died subsequently in Lucea, so that the half or two-thirds must have succumbed to the destroyer. At Orange Cove, an estate between Lucea and Lance's Bay, seventy-five out of a population of 100 were cut off, the overseer being one of the number. But, at Batchelor's Hall, seventy out of seventy-three of the residents perished, being a mortality of ninety-five per cent. As there was a medical man residing on the estate, this excessive mortality cannot be ascribed to the absence of remedial means. Nor can the severity of the attack be ascribed to any local cause, the estate being situated on a calcareous plateau, three or four hundred feet above the level of the sea, and on the edge of a precipitous cliff washed by the waters of the mighty Atlantic. The huts, also, were clean and well-built and perfectly ventilated, so much so, that they

would have delighted the heart of an out-and-out sanitarian.

While, on the one hand, the ravages of epidemic cholera have been greatest among scattered populations and in small towns and villages, so, on the other, they have been least in the towns with the densest populations. Nowhere was this result more clearly demonstrated than during the outbreak of cholera in London, in 1849. This will be evident by a reference to the following table compiled from the Registrar-General's Report for 1849.

TABLE A.—*View of the area, density of population, and mortality from cholera, and all diseases, in three groups of districts, in 1849.*

	Localities.	Population.	Persons to an acre.	Ratio of Deaths per cent.	
				From Cholera.	From all causes.
1	47 Districts, including the principal sea-port Towns	2,153,319	9	·85	2·53
2	41 Districts, including the principal inland Towns	2,243,183	13	·38	2·48
3	London	2,361,640	243	·62	2·57

If we examine the above figures we shall find, that the mortality from cholera was greatest in the district where the population is the most scattered, viz. nine persons to an acre, while it was, with one exception, the least where the population is the greatest, with 243 persons to the acre. Instead of comparing large towns with country districts, we will take the different districts in

London, in which the hygienic condition of the inhabitants is the same, the only difference being, that the density of population varies in each, and then observe the result.

In the above instances, the mortality from

TABLE B.—*Tabular view of the density of population, with the mortality from cholera and other diseases, in certain districts of London, in 1849.*

	Districts.	Number of persons to an acre.	Deaths to 10,000 inhabitants.	
			From Cholera.	From all causes.
1	East London	282	45	252
2	St. James, Westminster	222	16	212
3	Clerkenwell	202	19	242
4	St. George, Southwark .	181	164	267
5	St. Saviour, „ .	141	133	292
6	St. Olave, „ .	114	181	281
7	Bermondsey	66	161	264
8	Lambeth	34	120	233
9	Rotherhithe	19	205	277
10	Wandsworth	4	100	198

cholera, as is evident, was greatest in the district where, with one exception, the density of population is the least, while it was least in that district where, with one exception, the density is the greatest. Then, again, if we take the district in which there are only four persons to the acre, we find, that the mortality was more than double what it was in the district where there are 282 persons to an acre. With these results before our eyes—results that are diametrically opposed to the doctrine of contagion—it

would seem to be alike illogical and childish to talk of the infectious nature of epidemic cholera. More than this, it appears to me, that it is not only childish but a sin and a crime for medical and scientific men to inculcate doctrines that, when imbibed and believed in, are productive of the most serious and disastrous results in a social, political, moral and commercial point of view.

Another new disease sprang up almost contemporaneously with epidemic cholera, which is believed to be contagious by certain writers. This is typhoid fever, a disease unknown in Europe until 1812, and in England until 1826. At first it was concluded, and especially by sanitarian writers, that this malady was a product of the decomposition of organic matter, although it seems difficult to understand how a new disease could be produced by a cause that must have been in operation from time immemorial. Hence, as Dr. Farr remarked, "typhoid fever or typhia as it may be termed to distinguish it from typhus, Dr. J. Smith, Dr. Murchison, and others, have shown to be a kind of night-soil fever."⁴ As such, Dr. Murchison proposed, that it should be called pythogenic fever, i.e. the product of putrescence. Referring to the outbreak of typhoid fever at Terling, in 1867, Mr. Simon remarked: "The cause of all this terrible sickness and mortality was, of course, evident to their lordships' inspector at a glance. It was the merest question of *filth*."⁵ So, again, while commenting on a severe visitation of typhoid fever at Windsor, the same writer added: "The disease

⁴ Registrar-General's Report for 1859.

⁵ Report to the Privy Council, 1867, p. 28.

was one of those common filth-fevers which occasion, in England and Wales, annually, about 18,000 *preventable* deaths.”⁶ Yet Windsor is a remarkably clean town, and it had been well and efficiently drained a short time before. If this disease be caused by filth only, it ought only to prevail in insanitary towns and places, but no such distinction has been observed. It is not surprising, therefore, that other writers have ascribed typhoid fever to other causes—to contagion. This was the case, more particularly, with the late Dr. Budd, who wrote a work expressly to prove the contagious nature of typhoid fever. “The living human body,” says the writer, “is the soil in which this specific poison breeds and multiplies.”⁷ Although Dr. Budd states that the virus is spread by tainted hands, by linen, bedding, &c., as also emanations borne on the air, he concludes, that the internal discharges constitute the material by which the fever is mainly propagated. This doctrine, strange as it may appear, was at once adopted by Mr. Simon, who remarked: “The facts, which Dr. Budd adduces, from his own experience and from that of others, are, in my opinion, sufficient to prove that the contagion of typhoid fever is *importable*(?) by persons who have it.”

After this change of front, to use a military term, on the part of the great apostle of sanitary reform, it would hardly seem necessary to adduce other proof, in order to show that the first of these theories is an erroneous one. There are, however, certain facts, which it may be as well to mention, that prove most conclusively the un-

⁶ Id. 1859.

⁷ *On Typhoid Fever*, 1873.

soundness of this theory—the filth theory. During a discussion that took place at the Social Science Association meeting, in 1861, on sanitary reform, or in other words, the theory that ascribes the production of all diseases to the decomposition of organic matter, Dr. Christison remarked: “Were this a well-established principle in social science, the extinction of so deadly a malady—typhoid fever—should be no very difficult matter.” It has, however, proved a very difficult matter, so much so that the very attempts made to prevent the rise of this and other diseases have not only failed, but would seem to have produced the opposite effect. The instance of Windsor has been already mentioned. Dr. Bennett, at the meeting of the same society, in 1862, while referring to the prevalence of typhus and typhoid fever, observed: “The latter has recently appeared in Edinburgh, and, strange to say, it appears to have *followed the improvements.*” And Dr. Christison, the president, added: “Our street-drains in the old town have been much improved, *during the very period* that enteric or typhoid fevers have been increasing. . . . Further, this disease does not by any means generally break out where the streets are ill-drained, water-closets wanting, and habits filthy.”⁸ On the contrary, the reverse would appear to be the fact. This was the case at Lewes, in Sussex, which had two outbreaks of typhoid fever, in 1872 and 1874, shortly after new drains and sewers had been made, and a fresh supply of water from a high, unpolluted source, had been obtained. No such outbreak of fever had been known previously, Lewes having been from time immemorial one of the healthiest towns in the

⁸ Report, 1861.

kingdom. Other instances of the same kind have occurred, but it is unnecessary to produce them now. It is sufficiently evident, from the preceding facts, that typhoid fever, is not, and cannot be caused by the decomposition of organic matter, and, more especially, of the excreta of man. This is a conclusion at which we might have arrived for other and different reasons. Were this theory true, typhoid fever would prevail to a greater extent in towns than in country districts. But the reverse is the case. Dr. Acland, referring to a visitation of typhoid fever, in the parish of Great Horwood, and its great prevalence, remarks: "An endemic, affecting a similar proportion of the population of London, would attack, in nine months, more than a quarter of a million of persons of all ages; above 50,000 would die of the fever alone, while the deaths, from other causes, would not diminish."⁹ No less conclusive is the following circumstance. An outbreak of typhoid fever occurred on board the school-ship *Cornwall*, moored in the Thames off Purfleet. The first case observed was in the middle of September; but the disease did not assume an epidemic form until the 4th or 5th of October, when the cases increased so rapidly, that there were thirty-nine, including one of the officers, on the 16th of the month. As there are neither water-closets, cesspools, drains or sewers on board ship, while washing and scrubbing are carried out even to excess, there could be no accumulation of filth here. For the same reasons, the water could not have become contaminated, independently of the fact, that it is always stored in closed reservoirs.

While this filth-theory is thus shown to be alike unsound and illogical, the conclusion, that the origin and spread of

⁹ Fever in Agricultural Districts.

typhoid fever is to be referred solely and entirely to contagion is no less so. Even the severe, continued, and remittent fevers of intertropical climates are not so regarded, neither are measures ever taken to guard against infection. No instance is known of sailors, who contract fever on shore, giving it to their mess-mates when removed to their ships, although they sleep in the midst of the healthy; nor do soldiers when attacked with fever at outposts, or on guard, infect their comrades after removal to barracks. That typhoid fever is a malarious fever may be shown from the fact that it has replaced intermittent fever, and is the result of drainage of the soil. "The drying or drainage of the soil," observes M. Boudin, "or its conversion into a lake, while producing disappearance or diminution of paludal diseases, or rather of intermittent fever, appears to predispose the organization to a new pathological condition, in which phthisis and typhoid fever play a prominent part. It may be said, however, that ague is only observed in a few isolated districts in England, while typhoid fever prevails generally. This is no doubt the fact, at the present time, but ague was formerly as prevalent in England, as it is now in Italy and Spain. It prevailed epidemically as late as the middle of last century, and raged like a plague, being very fatal.

That the disappearance of ague is to be referred to the cause now named, and that it has been replaced by phthisis, and typhoid fever, admits of no doubt, for we have observed this process and its results in the present day, and under our very eyes. For instance, the town of Winterton, Lincolnshire, is situated on high sloping ground, and built on the oolitic formation, which, being

porous, renders the natural drainage good. Notwithstanding, in 1863, a thorough and efficient drainage of the town was carried out, with what result the Government inspector will tell us. "Ague," says Dr. Thorne, "was up to thirty years ago, very prevalent in the district; but since the neighbourhood has been properly drained no cases have occurred."¹ Exactly so, they have been replaced by typhoid fever, which prevailed slightly in the two previous years and severely in 1865-6 and again in 1867. Take another example. Mr. Cass, surgeon of Goole, states, that thirty-seven years ago, when he first went there, intermittent fevers were very frequent, but not often severe in form. About this time extensive drainage of the land in this district was effected, the result of which has been told by Dr. Whitley, medical inspector, who remarked in his Report, "There has been much typhoid fever of late years, and much zymotic disease prevailed at the time of my visit."² Mr. Cass added, "the mortality from phthisis has been very high, for many years." This phenomenon, a change in the type of diseases, from the drainage of the land, has been observed from time immemorial. Thus, the marsh called *La Chartreuse*, near Bordeaux, was drained in 1805, immediately after which a fever broke out, of so severe a character, that 12,000 persons were attacked, and 3000 died. The same result has been observed in the Pontine marshes, in the drainage of which vast sums have been expended, only to render the plains, where large cities formerly stood, uninhabitable. With these

¹ Mr. Simon's Report, 1867, p. 28.

² *Id.* 1863, p. 444.

facts before us, we are bound to conclude, that typhoid fever is a product of malaria, if so, it cannot be a contagious disease.

That it is not propagated by contagion may be concluded from the fact, that this disease prevails more in country districts than in towns.

Another source of infection has been lately discovered, or supposed to have been discovered, by the contagionists; I allude to what is termed "The Milk Theory" of disease. It is desirable, therefore, to ascertain on what foundation this theory rests.

For this purpose I will take the outbreak of typhoid fever in Marylebone, which was referred to this cause. It appears from the Report of Dr. Whitmore, Medical Officer for Marylebone, that on August 4th, 1873, Dr. Murchison called on him, and stated that four of his children were suffering from enteric fever; and that the disease had also broken out in several families in the immediate neighbourhood. The cause of it he suspected to be the milk supplied by the Dairy Reform Company; as it was ultimately found, that nearly all the persons attacked had their milk from the same dairy. The conclusion was at once drawn, that this was the source of the infection. Although the warning has been so often given, that coincidence is not cause—if it were, we might, as Sir John Herschel has quaintly remarked, consider the night to be the cause of the day, or *vice versâ*, for these two phenomena are inseparably connected together—this is the rock on which medical theorists are continually wrecking their own reputation, and the welfare and interests of the public. But to proceed. An inquiry was at once instituted, conducted by Dr. Whitmore,

Mr. Radcliffe, from the Board of Health, Dr. Murchison, and others; and the following facts were elicited.

The company derived their supply of milk from eight different farms: and as no possible source of infection could be discovered in the dairy in London, it was resolved to visit all these farms in succession. "In seven of these farms," says Dr. Whitmore, "the investigators were unsuccessful in discovering the cause of the outbreak. . . . On the eighth farm, however, the condition of things, which then existed, coupled with some antecedent facts, which had come to our knowledge, on the evening before we made our inspection, demonstrated *beyond the possibility of any reasonable doubt*, that the fountain and origin of the epidemic had had been at last found out. This farm, known as Chilton Grove Farm, is situated some few miles from Thame, Oxfordshire. It had been in the occupation of a Mr. Jessop, who died on the 8th of last June, since which time it has been carried on by his widow From information furnished to us by Mr. Humphreys, surgeon, Thame, the medical attendant of Mrs. Jessop and her family, we learned that Mr. Jessop, some short time before his death, was attacked with typhoid fever; and although the return made to the district registrar ascribed the cause of death to 'heart disease,' yet, from the circumstance of Dr. Giles, of Oxford, whom he consulted in the early stages, having pronounced it to be typhoid, as well as from the additional fact, that, some two or three days before he died, copious discharges from the bowels—characteristic of the disease—had taken place, it is very clear that this gentleman had suffered from the fever in an aggravated form, and that a cer-

tificate, ascribing his death to 'enteric or typhoid fever,' would have been more correct." Here then we have two or three or more gentlemen, who never saw the patient, taking upon themselves to affirm, that the prognosis of his medical attendant was erroneous; and that he died of a different disease to that which the medical certificate affirmed. And why? Merely because he had hæmorrhage from the bowels, previously to death. But this effect, *per se*, was no proof of the disease being typhoid; for it is equally characteristic of heart disease, of disease of the liver, and certain chronic affections of the bowels. This, in fact, was the cause of the death of George IV.; but there was no talk of typhoid fever in his case. As to Dr. Giles, who only saw the patient at the commencement of the attack, he could not possibly have formed as correct an opinion of the case as Mr. Humphreys, who watched the patient from the commencement to the termination of the illness. Having, however, arrived at the above conclusion, it only remained to ascertain the way in which the patient, who had died exactly two months before, had infected so many persons in London. When evidence is required of a certain nature, testimony, in accordance with the ascertained views of the inquirer is generally forthcoming; and so it was on the present occasion.

It appears, that the farmhouse and buildings are placed on a slope; the privy being at the highest point—at an altitude of about eight feet—and a well at the lowest. The well, which is bricked, but not cemented, and received its supply of water from a brook a short distance off, is distant from the privy between sixty and seventy feet. Up to the time of the visit, the water in the well "had

not been used for drinking for nearly two years, but simply for cooking, washing, and for washing out and cooling the pans." Such being the condition of the premises, "it was obvious," adds Dr. Whitmore, "to myself and the other gentlemen present, that the water was impregnated with the soakage from the privy, and also from the pigstyes and manure heap, which were even in much closer proximity to it, and even upon a higher level. The theory, therefore, by which the infection of the milk is to be explained is as follows: At some time, probably for some days during the illness of Mr. Jessop, his excreta, *intensely impregnated* with typhoid poison, and mixed with other liquid, and decomposing animal matter, was conveyed into the well by percolation; the water of this well, thus poisoned and rendered still more dangerous by drainage from the pigstyes and manure heap, was daily used for washing out the cans; and it is very probable that, after such washings, some small quantity was left accidentally at the bottom of them, and on pouring milk into these cans the whole of it became at once infective."

Such is the theory, now for the facts. Allowing, for the sake of argument, that Mr. Jessop died of typhoid fever, and that the excreta of the patient were thrown into the privy, "it was," as stated by Mr. Emery, at a meeting of the Marylebone vestry, held October 2nd, 1873, to take into consideration the Report of Dr. Whitmore, "a physical impossibility for the sewage-matter to get through eighteen yards of clay to the well—that being the distance from the closet, and there was never any overflow." Then, again, is it within the bounds of probability, that any water should have been left in the cans

after they had been washed out ; or, that a drop or two of water could have been so infective as to have rendered the milk placed therein thus poisonous ? Why ! prussic acid itself would have been rendered innocuous after such dilution. But all these questions are at once settled by the fact, that “the discharges from the body of the patient were *buried in a field* ;”³ where all diseased germs would be decomposed, by contact with the carbon of the soil. They could not, therefore, have found their way either into the brook or into the well. Had it been otherwise, however, the theory would have failed to account for the outbreak in Marylebone. To have rendered the conclusions valid, it would have been necessary to show, that only those persons were attacked, who had taken the milk from this one farm ; but no such evidence was offered. On the contrary, it was impossible that all the persons attacked could have drunk the milk from this one farm—for it was not mixed, I was told by the secretary of the company, with the milk from the other farms—as there were, it was calculated, at least 500 cases in the districts supplied by this company. Thus, of forty-three families, whose cases were inquired into, three did not have their milk from the suspected source ; while, of fifteen patients in the Middlesex Hospital, *three* had not taken any of this milk. Then, again, there were cases in other districts of London, not only at the time, but before and subsequently to the outbreak, and even before the individual, accused of all this mischief, had been attacked. There were even cases in Marylebone before this, as will be evident by the annexed Table of Deaths, compiled from the returns of the Registrar-General.

³ The *Hcur*, September 20th, 1873.

TABLE H.—*Number of deaths from typhoid fever, in the following months, and in the undermentioned districts.*

Month.	West District.	North District.	Central District.	East District.	South District.	Total.
May . . .	8	15 ⁴	4	16	17	60
June . . .	7	10 ⁵	6	10	14	47
July . . .	12	11 ⁶	6	11	18	58
August . .	22	30 ⁷	13	10	24	99

Other and important facts may be gleaned by an examination of the above Table. As the cases would bear a nearly exact ratio with the deaths, it appears, that the number of cases, not only in the other districts of London, but even in that of Marylebone, was as great in the month previous to the death of Mr. Jessop (June) as in that subsequent to his death, when the outbreak, it was supposed, had occurred. Then, again, although we are told, that this poisoned milk produced such disastrous results in Marylebone, it nevertheless appears, that the deaths from typhoid fever in the south district, where this cause was not in operation, were actually more numerous than in the north, or Marylebone, district. It is also apparent, that the number of deaths in Marylebone for the three months—June, July, and August—when alone the milk, if infected, could have had any influence, bears only a small proportion to the general mortality, in London, from typhoid fever.

In addition to these facts, there were cases of typhoid

⁴ Of this number 4 were in Marylebone.

⁵ 5 in Marylebone.

⁶ 4 in Marylebone.

⁷ 9 in Marylebone.

in London in all the preceding months of the year, and in all the districts. In April, the deaths amounted to 47; in March, to 68; and, in February, to 55. If we add the deaths in May to these, it will give a total of 230 deaths, and, probably, of 4000 or 5000 cases, not one of whom could have derived his disease from the alleged source of infection. What a farce, then, to talk of the residents in Marylebone having been infected from the milk they drank! It is clear, that there was some general cause in operation to which these persons succumbed the same as others. What this cause is, it will not be difficult to ascertain. Typhoid fever, as has been pointed out in the first part of this work, and more particularly in a subsequent one,⁸ is a product of malaria, for it has replaced intermittent fever. "The drying (or drainage) of the land," remarks M. Boudin, "or its conversion into a lake, while causing the disappearance or diminution of paludal diseases, appears to predispose the organization to a new pathological condition, according to the locality, and in which phthisis and typhoid fever play a prominent part."⁹ These conclusions granted, there is an end of the question, for no one, who has had the least acquaintance with this class of diseases, would dream of considering them contagious. A peasant in Spain or in Italy would laugh at the notion.

Although it is thus clear, to the commonest understanding, that the milk supplied by this particular farm had nothing whatever to do with the outbreak of typhoid fever in Marylebone, the whole town was alarmed; people

⁸ *The Antidotal Treatment of Disease.* Chapter, "Typhoid Fever."

⁹ *Étude de Géologie Médicale*, 1848.

were afraid to partake of any kind of milk; infants and young children were deprived of their natural and best sustenance: the owners of the suspected farms were unable to sell their milk, and the sale even of unsuspected liquid was all but suspended. This is not all. "To carry the matter further," remarks one writer, "and leave us without a shred of comfort, a learned professor now comes forward and assures us, that cows, which are themselves free from disease, may give milk which has undergone a poisonous alteration, owing to new principles having been formed by some obscure vital process. An animal that is not injuriously affected, may give off highly noxious secretions. This is a kind of information for which we feel by no means grateful to men of science. It increases neither our happiness nor our safety, but belongs to that description of which the wise man of old said, that he who increased it, increased sorrow. The most practical, and, therefore, the most valuable, piece of advice which occurs in the literature of this Marylebone outbreak is, that milk should be boiled before it is consumed, and we accept it with as much thankfulness as if it were new. But if we might ask a favour of the able men, who are just now enlightening us upon sanitary affairs, it should be, that they will principally consider and help human weakness, and not terrorize us with their superior knowledge. We have seen warnings heaped upon warnings, and precautions dictated after precautions, the minute and never-ending observance of which would make life not worth having upon such terms."² The injury to science is equally great. As one writer has truly remarked, in the sciences a false conclusion is dangerous, not so much

² The *Daily News*, August 25th, 1873.

because it propagates error, as from the fact, that it prevents or retards the search after truth. It is time, therefore, that we abandoned all false conclusions, acknowledged our ignorance like honest men and humble seekers after truth—it being, in general, egotistical pride that is the great bar to improvement—and commenced an inquiry *de novo* into the cause—the real, the efficient cause—of epidemic diseases.

THE END.



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